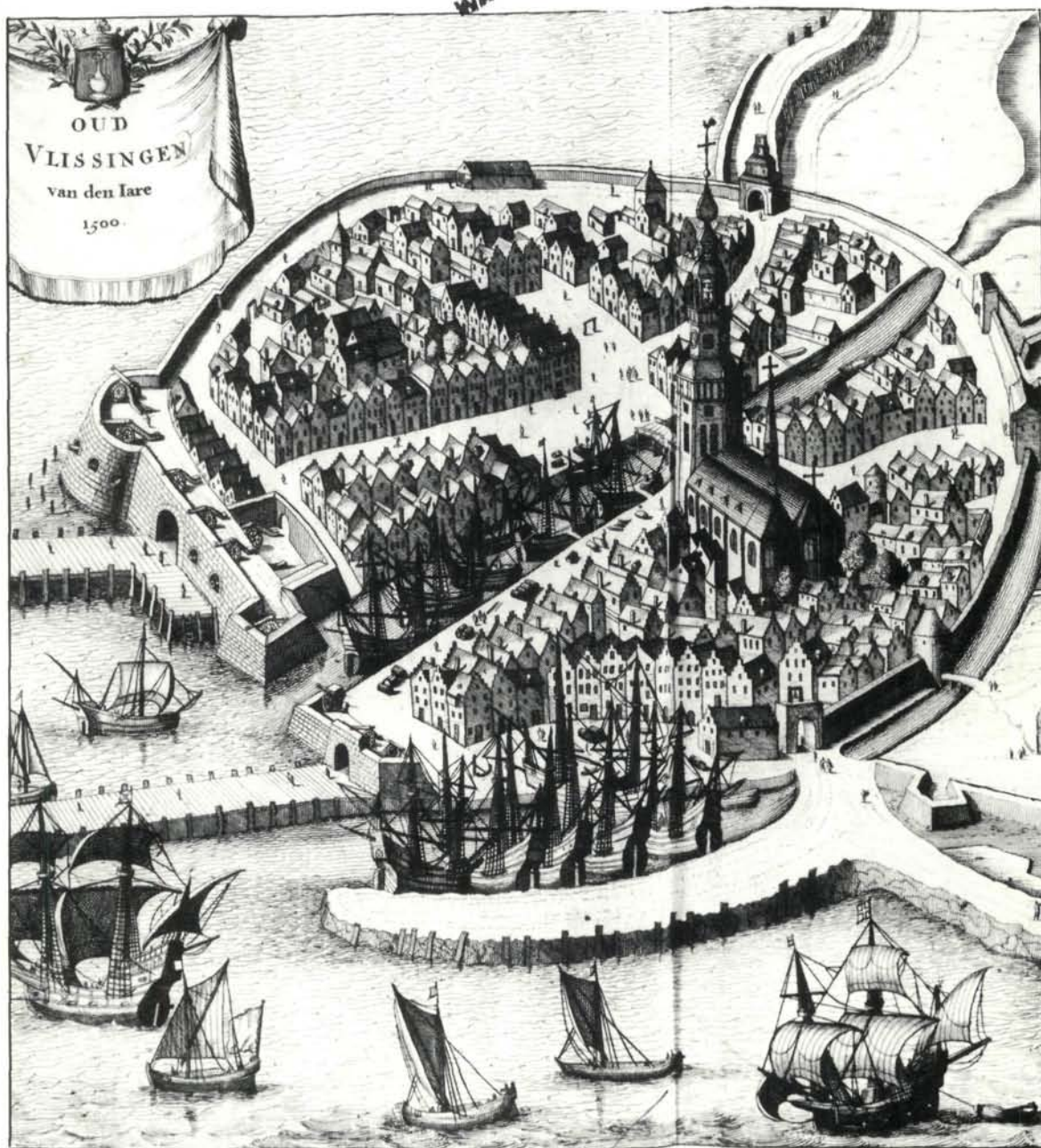


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*The American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003, and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$40.00 annually; \$20,000-\$29,999, \$35.00; \$15,000-\$19,999, \$30.00; \$10,000-\$14,999, \$20.00; below \$10,000 and joint memberships, \$10.00; associate (nonhistorian) \$20.00; life \$650. The proportion of dues allocated to the *AHR* is \$15.00. Subscription rates effective for volume 81: Class I, *American Historical Review* only, United States, Canada, and Mexico \$35.00, foreign \$38.00. Further information concerning membership and subscriptions is contained in the two pages immediately preceding the advertisements. Information concerning the ordering of back issues and the submission of manuscripts will be found on the page immediately preceding the advertisements.

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# The Idea of Authority in the West

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LEONARD KRIEGER

MY REASON FOR INQUIRING into the history of the idea of authority is neither an affection for authority nor the detection of a gap in historical knowledge that needs filling merely because it exists. Instead my purpose is to illuminate our much-discussed crisis of authority, the crisis which became particularly visible with its spread from the society at large into the groves of academe during the sixties and which is one feature of those tumultuous years that has survived the apparent return to normalcy.

This crisis of authority in which we now find ourselves is a crucial present concern for us all, whatever our political and social persuasion, because authority has been an essential means of controlling our collective destinies, and the crisis in it entails a crisis in our means of control by anyone and for any purpose whatsoever. It is a crisis, moreover, which is not only difficult for us to resolve with the means at our disposal, but is also difficult for us to understand with the knowledge at our disposal. Since the crisis of authority raises a problem of knowledge which apparently defies solution in contemporary terms, there would seem to be nothing left to do but to make a historical inquiry into the idea of authority for the purpose of shedding light on its present crisis. The social scientists, after all, have long told us that history is a residual discipline, so we might as well make the most of it.

Now let us make clear at once what the problem of knowledge raised for us by the crisis of authority is, since the nature of the problem determines the nature of the inquiry. The most obvious view of the crisis is to see it as the recent erosion of authority, and the object of an inquiry into it would be to watch it wear away. This view of the problem implies a quantitative approach to authority: either you don't have it or you do, and if you do you can have more or less of it. The history of people having less and less of it would tend to take the form of telling how every year a locust came and took another grain of wheat—a story which might be true enough but is neither very interesting nor very illuminating. It leaves open, moreover, the question of whether we are over-reacting to a transitory ripple or witnessing a fundamental change, and on this we find the contemporary evidence confusing enough to drive us back into history for clarification. Those who challenge authority both reject it as such on the ground that there is no valid distinction whatsoever among

humans to justify it—a rejection which seems the logical conclusion to the fundamental process of democratization—and reject specific authorities on the ground of their “illegitimacy”—a rejection which implies a remediable aberration and essential acceptance of authority as such. Among those who resist this challenge we find the crisis depicted *both* as the ultimate revolt against free variety in human nature and civilized form in human creation—a depiction which implies the fundamental barbarization of the culture—and as another chapter in the long cycle of generational revolts of the sons against the fathers—a periodic surge made somewhat more violent than usual by the transitional effects of affluence, technology, and a noxious war.

What we look for in history to resolve these ambiguities about the essentiality or contingency of the contemporary challenge to authority are the projective tendency and the projectile force of the long-range process in which authority has been involved. To abstract this tendency and assess this force we need a history whose span is protracted enough and whose stages are continuous enough to yield an identifiable and measurable momentum. Authority certainly has the required range, since in its broadest sense it is as old as human community itself. But for the required continuity we shall focus on the *idea* of authority. Whether the real institutions of authority are continuous from age to age or are discontinuously created afresh by the people of successive generations is a matter of interpretation, and contemporary opinion increasingly supports the interpretation of discontinuity. The continuity of the idea of authority, however, is a matter not of historical interpretation but of historical fact. It has moved demonstrably with the transmission of language itself from generation to generation since the time of the Romans—not only within the same linguistic nationality, but, with the historical diffusion of languages, from Latin into the vernaculars of the Romance tongues, and from French into English, and with the spread of Western culture, as spread from southwestern to northern and eastern Europe, and from Latinized terminology into Germanic and Slavic usage.

Obviously, the first step in an inquiry like this is to have clearly in mind what the idea of authority is, so that we may recognize it when we see it in its various historical guises and contexts. Now this is trickier than might at first appear. We can agree that authority objectively refers to a kind of relationship between members of a society and subjectively refers to a quality or capacity in certain of the members who are active parties in the relationship. But when we go further to ask what this relationship, quality, or capacity is we run into a real problem of definition—to wit, when we try to nail it down, the idea of authority seems to mean too many things to tell us much about our crisis or its background. Recourse to the dictionary does not help, for the common usages of the term, far from resolving, merely illustrate the problem of plural meanings. The verbal meanings range from power and dominion to influence and prestige. This span covers and obscures the distinction between force and persuasion, a distinction which is absolutely crucial both for the understanding of our own times and for the history of political and social ideas.



In the history of ideas generally, basic conflicts of principle often take the form of mere shadings of emphasis and proportion. In this sense the ethereal realm of the history of ideas resembles travel in outer space, where deflections of very small degree get magnified into fateful discrepancies covering vast distances when they are extended to actual contacts on this earth. Hence it will not do simply to expand our definition of the idea of authority to include what is common to all variations, since such a definition glosses over distinctions which must not be glossed over if the practical life of this world is to make any sense. If we combine all the meanings of authority into the definition of it as the acknowledged superiority of some men over others and write a history of it vis-à-vis the obviously contrary idea of the acknowledged equality of every man with all others, we shall miss the real historical problem in the idea of authority—that the choice of emphasis on the acknowledgment or on the superiority within the abstract definition of authority has entailed the use of the same idea to cover relationships both complementary to human liberty and opposed to it, both complementary to reason and opposed to it, both opposed to power and complementary to it. We need a definition of the idea of authority which will permit us to understand its internal dialectic.

Fortunately, historical definitions are available to give us the help which a categorical definition cannot give. In the early stages of the idea's development its predominant meaning was the acknowledged capacity of certain persons to evoke from others a voluntary submission to acts and opinions over and above the compelling force inherent in those acts and opinions themselves. In its later stages the idea of authority has come predominantly to mean the acknowledged capacity of certain persons to evoke from others submission to imposed acts and opinions by any means whatsoever. In its early form authority was an independent idea, lying between power and freedom in the spectrum of human relations; it was a pressure upon men to conform in ways to which they could not be ordered or compelled by the possessor of power and in things to which they did not freely consent or contract for good reasons or adequate compensation. Authority in this sense was, as has been well said by a famous historian of Rome, less than a command and more than advice. Those subject to this kind of authority, formally equidistant between power and freedom, tended to stress the side of authority which was distinct from power because it abjured force and which was complementary to free assent because it needed a *voluntary* obligation. In its later form, however, authority became a dependent idea, a quality of power connoting merely its legitimacy or rightful title to command and to compel obedience to its commands.

When we investigate it historically then, we cannot simply assume a constant idea of authority—say, as the acknowledged capacity to dominate others—and ask who claimed it, how people felt about it, and how men related it to other ideas, because the idea itself changed with the kind of people who claimed it and the way people felt about it. From a historical point of view we must rather look for the two basic forms of authority—the inde-

pendent capacity to dominate without compulsion and the legitimate power to dominate, including compulsion—and understand the process that led from one to the other. Now it would be convenient if I could say that this process—the process from moral authority to authoritative power, to put it in a crude package—is *the* historical definition of the idea of authority and that our task therefore is simply to look for the moral kind of authority in the earlier stages of Western history, watch for the point of transfer, and then work with the definition of authority as a dimension of power.<sup>1</sup> This is generally our procedure in tracing historical change over *shorter* spans of time, but in the *longer* runs the historical process is not so conveniently linear.

Certainly there has been a strong trend of this kind leading from one kind of authority to the other, but it is a case of too much of a bad thing. The trend has been so strong that the autonomous idea of authority has developed into the mere legitimation of power not once, but several times. It has happened, indeed, in each of the major periods of our culture since Roman times, thereby adding a cyclical pattern to the overall development. Between these cycles, moreover, there has also occurred and recurred a reverse process, a kind of countercycle in which men shift their attention from the stabilized authority of power to the dynamic claims of an independent moral authority. This countercycle cannot be considered a part or stage of the main process, because it entails a process of an entirely different kind. Where the original cycle is a development, the countercycle is a mutation. What this historical pattern shows, in fact, is that we have to deal with two recurrent processes and two persistent ideas of authority. Because they are involved in two processes which are not only reverse but incongruous, moral authority and authoritative power have not been two stages of one idea of authority, but two different ideas of authority immersed in a variety of historical relations with each other. At any point in time the idea of authority is not *the* idea at all; it is actually a composite attitude based on the relationship that exists between the two ideas of authority at that time, and this relationship, in turn, is based on the respective stages which these ideas have reached in their separate histories.

If, then, we are going to bring history to bear upon the contemporary attitude toward authority, we have two ideas to follow in the past. Only when we have sketched them both, in their own terms, and matched the current impetus of each with the other can we draw lessons of history for our current crisis of authority.

LET US FIRST ESTABLISH THE HISTORICAL PATTERN assumed by the idea of authority that is familiar to us because it has become predominant in modern times. This is orthodox history in which we start with the end-point of

<sup>1</sup> The abbreviated reference to the early, independent stage in the idea of authority as "moral authority" should not be taken to imply that the contrary ideas of power and freedom did not have moral dimensions of their own. Neither these ideas nor their distinctive morality will be discussed here.

development—in this case a developed present idea—and ask the past how it grew. This idea of authority is an idea in the sense that it is a general notion that historical people actually and consciously held about authority. According to this idea, authority is simply constituted power—that is, any capacity to secure obedience or conformity that carries with it some title to do so. It is located primarily in societies which are overtly political or which harbor a coercive force akin to politics, since only there do people think about power directly. This idea of authority, then, is a solid idea—a nugget of an idea. For its past, one asks how it came to be the idea of authority out of what was not the idea of authority, how big it was at any time, who held it, and what titles they gave for holding it. Its history is not a history of changes *in* the idea, since such a simple idea cannot change without becoming something else; it is the history of the initial crystallization out of the intellectual nebula *into* an idea and of the subsequent changes in the *external* relations of this idea with practical circumstances and with other ideas.

This idea of authority as a consciously constituted or legitimate power to command and to secure obedience is a modern idea—that is, it is one of the ideas whose distinct emergence during the sixteenth and seventeenth centuries have led historians to mark the period as the origin of modern times. Certainly men had acknowledged authorities *in practice* since the beginning of organized human society, but in too many contexts and for too many different purposes to require any thought about authority as such. Certainly too there had long been plural ideas of authority, connected with these specific institutional authorities and usually ensconced in traditions, which would persist in the form of corporate or constitutional notions throughout the modern period. The point about the generic idea of authority as legitimate power was not its prevalence but its appropriate innovation in the early-modern age.

At the beginning of the modern period, there was indeed a city-state tradition, which had been recently revived by generations of humanists and which perpetuated the classical notions of political power, but in a local context and with moral overtones which relegate it to the prehistory of the idea of authority. There was, similarly, a medieval territorial tradition which posited a harmonious order of superior, subordinate, and coordinate authorities, but within a general acceptance of plurality and a presumed cooperation for a common religious purpose that relegate it also to a prefatory state. Convulsive events of the early-modern period had the effect of dissolving the classical and medieval protective covers which had preserved the limited and fragmented power of authority. Machiavelli showed what could happen to the protective cover of the city-state's fabled civic virtue when political attention was shifted to the larger arena of the territorial nations which made it an anachronism. And later in the sixteenth century Jean Bodin showed what could happen to the protective cover of a common religious purpose when he developed the doctrine of a lawfully constituted, supreme, indivisible, exclusive, and purely political sovereignty in response to the raging wars of religion that were triggered by the conflict between Christian churches but fed by the

espousal of a religious cause on the part of every authority with a claim to more power.

The natural-law writers of the seventeenth century—Grotius and Hobbes, most prominently—added the theoretical foundations to the definitions advanced in the sixteenth, and from this sequence of responses to the mutually destructive conflicts among the traditional hybrid authorities there emerged the modern idea of authority as legitimate power *per se*, distinct from any of the rights that constituted it or the ultimate ends to which it might be directed. It signified the segregation of politics as the sphere of common interests on whose uniform administration the community could agree, because such unified control was a necessary condition for men's pursuit of the various social and spiritual ends on which they could no longer agree; and it signified the community's acknowledgment of power as *the* principle of this autonomous political sphere. Hence the modern idea of authority arose as the new built-in rationale of political power to replace the divisive social and spiritual purposes which used to justify it. In the period of its first definition—that is, in the sixteenth and seventeenth centuries—modern political authority performed this function precisely, both in fact and in idea. It was a single crystalized point of generally accepted power, politically raised above the myriad traditional social and ecclesiastical authorities—aristocracies and state churches—which still persisted but whose power relationships the political authority now took over.

From that time forward the application of this modern idea has been steadily expanded, homogenizing the various types of hierarchical human relations into cases of authoritative power and polarizing the world of political ideas into systems of authority on the one side and of freedom on the other. For clarity's sake, we can mark off this gradual process of simplification into two stages. In the first, the social world was simplified down to two linked ideas—authority *and* freedom. In the second, it has been refined down to an ultimate choice: authority *or* freedom.

The first stage in the extension of the uniform modern idea of authority was the response to the two urgent questions raised by the explosive new definition of authority as justified power: what was the extent of such authority (that is, what was the boundary of the common political interest for which the exercise of power was collectively recognized?) and who would exercise it? From the end of the seventeenth to the end of the nineteenth centuries men devoted themselves to these questions, and beyond all the local permutations and combinations in the answers the general theoretical result was for thinkers to align their answers into whole systems of ideas under the primacy either of authority or of liberty. This categorical confrontation, streamlining *authorities* into authority and *liberties* into liberty, testified to the simplifying and expansive effect of defining authority in terms of power, in contrast to the plurality of traditional authorities with their obscure frictions against one another. Indeed, what demonstrated the universal resonance of the new idea of authority above all else was that even the opponents agreed essentially on



what it was. Conservatives may have emphasized the legitimation more than the power and liberals the power more than the legitimation; and, of course, conservatives approved of authority while liberals feared it. But by and large they were on opposite sides of the same thing.

And yet this was only an intermediate stage in the expanding range of authoritative power. For if there was a general consensus that authority must have power within its due sphere, there was also a general consensus, on the part of authoritarians and libertarians alike, that the sphere of authority was but one of the two spheres which made up the social world and that the sphere of authority therefore was not by itself coterminous with this world. If we keep in mind the two patron saints of this period's intellectual conflict, John Locke for the liberals and Edmund Burke for the authoritarians, we get an accurate idea of the standard relations between the two leading ideological systems of this stage: the liberals sketched out a large sphere for political rights and civil liberty and a small sphere for a responsible authority, and the conservatives sketched out a large sphere for aristocratic—that is, independent—authority and a small sphere for freedom of enterprise and liberty of conscience; but both sides acknowledged some role for both the authoritarian and the liberal principle.

In the twentieth century this process of homogenizing ideas and polarizing the human world was taken one stage further to a logical extreme. In totalitarian fascism the principle of authoritative power became the principle of the entire human world. As Mussolini formulated it: "Fascism stands for liberty, and for the only liberty worth having, the liberty of the State and of the individual within the State. . . . It is the purest form of democracy if the nation be considered—as it should be—from the point of view of quality rather than quantity, as an idea . . . the truest, expressing itself in a people as the conscience and will of the few, if not, indeed, one. . . . Therefore the State is not only Authority which governs and confers legal form and spiritual value on individual wills, but it is also Power. . . ."<sup>2</sup> In the ideology of fascism, therefore, authoritative power did not reject liberty—since rejection would have implied the existence of an idea outside of authority to be rejected—but absorbed liberty into authority and converted liberty into a function of authority. Thus Hitler repeatedly insisted that in contrast to the rights of the majority in modern democracy, he espoused the principle of "old Germanic democracy, . . . which knows only an authority which proceeds downwards from the top and a responsibility which proceeds upwards from the bottom," and he defined this nominally bi-directional process as a real hierarchy which subsumed the democratic responsibility under authoritative power. The leadership might recognize morally "as its supreme instance the authority of the German people as a whole," but actually this recognition contributed to the people's "blind obedience" in "following the leader, . . . a single will which issues its commands and which the others must always obey."<sup>3</sup>

<sup>2</sup> Benito Mussolini, *Fascism: Doctrine and Institutions* (Rome, 1935), 11–13.

<sup>3</sup> *The Speeches of Adolf Hitler*, ed. Norman H. Baynes (New York, 1942), 1:180, 20, 200–02, 543.

Now the ideology, if not the reality, of totalitarian fascism died in 1945, and it might seem as if we have entered a post-mortem stage in the modern idea of authority—as if, that is, the trend which we have observed toward its continuing simplification has continued to the point where it has refined itself out of existence, along with the demise of all the other political and social principles caught in what has been called “the end of ideology.” It is tempting for an age as quantitatively oriented in its thinking as ours to infer that a development going from many kinds of authority to one kind of authority will naturally conclude in no kind of authority as the simplest idea of all. And undoubtedly the contemporary view of our age as pluralistic, practical, negotiable, open-ended, on the one side, and materialistic, group-oriented, and self-alienated on the other would seem to leave no important place or function for the idea of legitimate or justifiable power.

But there is another possibility. There is in the postwar intellectual movement an indication that the modern idea of authority did not reach its climax in the overt totalitarian regimes after all—that the totalitarians’ attempts to use the organs of state as their legitimate instruments for the exercise of total power over the society implied the persistence of a distinction between state and society, and to that extent did not carry the simplification of the idea of authority to its final conclusion. In the thinking of Herbert Marcuse, for example, the ultimate shape of the modern idea of authority emerges only during our own age, taking the form of a subtle power constituted in all of our social organizations and penetrating even into the contemporary “one-dimensional”—that is, submissive—individuals, who thus legitimate this single, pervasive, manyheaded authority within their very psyches. Such a conception uncovers a crucial possibility in the modern political idea of authority which is easily overlooked. We are accustomed, perhaps from the historical origins which we have discussed, to think of politics as a set of definite things (that is, governments) operating in their own distinctive sphere—the public. But in the context of authority politics is actually something more general which took this reified form in a definite historical period. The modern political idea of authority as such simply makes authority the principle of power over men. Consequently, when society gets politicized, as ours has been, it can take the overt totalitarian form of extending the coercive activities of governmental organs into society and thus enlarging the public sphere at the expense of the private, but it can also take the covert form of reorganizing voluntary associations into power relationships legitimized by the nominally voluntary base and thus of erasing the line between the public and private spheres so far as the principle of authority is concerned.

THIS IS AS FAR AS WE CAN GO with the history of our *modern* idea of authority. It is an ending reminiscent of the story about the lady and the tiger. It leaves us with the choice of all or none. It leaves us, that is, with the knowledge that history, in the sense of the tendency in the past development of the idea, leads

equally to either of the contemporary attitudes toward the idea of authority. This history supports both the idea that authority has expanded continuously until it has now become everything and the idea that authority has been focused continuously until in the concentrated form of post-fascist totalitarianism it disappeared into naked violence. The choice is rather important, since the first attitude toward authority fits a program of total revolution and the second attitude toward authority denies a moral target for any revolution. There would seem to be something wrong with a history that keeps saying “amen” to both of such choices. What we gain from this history is the understanding that there is historical reason for the confrontation. What this history does not tell us, since it is a history of the common idea of modern authority, is whether contemporary men are really talking about the same thing when they talk about authority or whether they actually have different ideas in mind.

As I indicated earlier, there is another idea of authority whose historical career has been quite the reverse of the modern one. Where the history of the modern idea shows a continuous development from obscure origins to a clear and definite concept, the history of the other, older idea of authority shows a recurrent pattern of regression from clear and definite origins to a present obscurity. Such a history means that we may well be carrying about with us connotations of authority which we are not quite aware that we have and that we may well be modifying the modern idea of authority with them in ways which distort our communication about it. To consider whether we are in fact bearing unbeknownst this burden of the past, we must do two things: we must return to the early period when this older idea of authority was most explicit in order to learn what the telltale signs are that we must look for in the present; and we must look into the subsequent history of the idea to determine the probability of its persistent underground effectiveness in our own age.<sup>4</sup>

For the seminal period in the history of this older idea of authority let us turn to the Romans, not because we necessarily believe that it was born there—although some scholars do believe that—but because the Roman use of one distinctive Latin term *auctoritas* for the several applications they made of the idea gives us the most convenient way of following what it meant for an ancient culture, and because the philological connection between Latin and modern authority makes the Latin version particularly available for infiltrating the modern idea of authority. Before we try to define what the Romans meant positively by authority, let us delimit it in general by specifying its two most prominent differences from the modern.

The Roman idea of authority evolved in contradistinction to power. The two most striking examples of this function were afforded by the Roman Senate and by Augustus, the first emperor. As opposed to the command power of the magistrates and the constituent power of the people the Senate

<sup>4</sup> For a more explicit and less analytical elaboration of the materials on which the interpretations in this section are based, see my entry on “Authority” in the *Dictionary of the History of Ideas*, ed. Philip P. Wiener (New York, 1973), 1:141–62.

was recognized as having only “authority,” a term signifying the Senate’s peculiar mode of domination which, unlike the other offices of the republic, had no legal status and no compulsory force, and yet in fact bound the Roman citizens to compliance far more definitively and extensively than did the other offices of the republic. As a further mark of this distinctive, uncoercive authority associated with the Senate, we may note that the Romans attributed “authority” to the Senators in their capacity as members of an advisory council of “fathers” or “elders,” but preferred to use the idea of “power” for the domination of the father within his own family. After the conversion of the republic into an empire Augustus expressed the distinction between authority and power when he characterized his own position as equal in power with the other magistrates but supreme in authority. We may pause, in passing, to note the two different contexts afforded by these examples for the same idea of authority, since clarification now may avoid confusion later. Authority could be possessed both by men without official power, like the Senate, or by men with official power, like Augustus, but in both cases it referred to the same kind of domination—a kind of domination which lay outside the exercise of power.

The second general distinction between the Roman and modern ideas of authority is that the Roman idea was multiple. It was a flexible idea with plural meanings and varied applications held together by an implicit common tendency. To revert to an earlier analogy, it was an idea with moving parts, which constantly shifted in their internal relationships with each other and in their external relationships with other ideas. We must be alert to these fluctuations as, in the course of time and changing circumstances, authority assumed new forms and acquired new sources.

For the *positive* content of this multiple idea we must also look to the Romans. They developed the three basic forms and the several kinds of sources of authority which recurred time and again in the history of the idea. The three basic forms of Roman authority were: authority as guarantee; authority as origin or creation; and authority as personal prestige. Sources of authority appropriate to it as guarantee were trustees who by declaration confirmed the acts of their wards and elders who by experience confirmed the acts of their people. Sources of authority appropriate to it as origin or creation were planners whose designs were carried out by others and initiators whose proposals were enacted by others. Sources of authority appropriate to it as personal prestige were nature’s noblemen—that is, men of recognized moral or intellectual pre-eminence. At first glance, these groups would seem to have little to do with one another. People who merely add guarantees to something else certainly would seem entirely different from those who initiate or create something; and both of these groups, who were authorities by virtue of something they *did*, seem quite different from the third group, who were accounted superior by virtue of something they *were*. And yet it was from combinations of these apparently ill-assorted ideas that the Romans derived some of our most familiar applications of authority. From the combination of

the authority that is due the initiator and the guarantee added by age and experience they derived the authority of tradition. From the combination of the authority stemming from an original design and the authority of personal pre-eminence they derived the long-lived idea of authority as the inevitable product of the natural inequality of men. From the combination of the authority due to intellectual pre-eminence and the guarantee of experience they derived the idea of the authority of the expert.

These Roman combinations, moreover, produced more than persistent general principles of authority; they also established long-lived concrete forms of authority. When the Senate combined the initiation of legislative proposals—still under the guise of extralegal advice—with its earlier function as an unofficial council of guardian elders, it became, despite its lack of power, the “principal” organ of the Roman constitution. This combination established the fateful double meaning of primacy in government that has endured: the right to act first in time is also the right to be first in rank; the authority with the function of initiative is the supreme authority. From the same combination of the original authority of the initiator and the added authority of the trustee the Romans derived the equally important concept of authorization, through which an original authority is not only transferable but is actually enlarged in the hands of the representative who becomes its guardian. From the second combination of the authority due a progenitor with the authority due the moral pre-eminence of a benefactor has come the influential application of natural hierarchy that has supported parental authority generically and paternal authority especially. And from the combination that produced the authority of experts as a class has descended the specific focus on the authority of the teacher.

These blends of authority are not only important ideas in themselves, but they indicate the connection underlying the apparently disparate forms of the older idea of authority. They all import some general claim on human trust into a social relationship in order to introduce an additional pressure for conformity beyond that which the relationship itself can exert. Indeed, we may say that, if obedience is the counterpart of power, trust is the counterpart of authority. What all these forms and sources of Roman authority have in common is that they are trustworthy. This is obvious in the case of the trustee and the personally honorable man, but a moment’s reflection will show that it is also behind what the identification of the creator contributes to the execution of his design; that is, it provides the executors with a principal agent who can be held accountable.

Just as revealing of this idea of authority as these internal relations were the two things that happened to it in Roman history. The first was that Roman law established the connections between these highly variegated forms of authority and molded them into *an* idea. References to authority appeared in all spheres of Roman life, but it was law, the sphere in which Rome invested so much creative genius, that became the matrix for the organization of the several materials of authority into a new coherent idea.

The second development was the change that the idea underwent along with general changes in Roman conditions. The original Roman idea of authority proved to be unstable. While independent of other ideas, it was internally fragmented. Subjected to opposing pulls from the idea of power, to which the theme of domination in authority made it kin, and from the idea of liberty, to which the theme of voluntary, uncoerced compliance in authority made it equally kin, it did not succeed in maintaining the delicate and tenuous balance between them that was the condition of its autonomy. During the course of the Empire the idea of authority became increasingly associated in Roman law with the power of the Emperor, until it finally became a quality of his power, prefacing his formal decrees as a kind of verbal symbol of their legitimacy.

Thus the Romans ended with the familiar, if to them still subliminal, idea of authority as authoritative power which we have inherited. But if we look more closely at the history which lies between them and us and if we look at it not for the further development of the familiar idea of authority they ended with but for the original idea of authority they started with, we find it reappearing time and again. If we look at this intervening history for a pattern of these recurrences we find that not only the result but the process of Roman history is relevant to us in the present. With each of the main creative surges of Western man, and in the specific field of the creation, the idea of authority was reborn as the means of diffusing the innovation among masses of routinized people and of organizing society in the light of that idea, but without violating the freedom of commitment which is in the spirit of each cultural creation. And in each case this fragile idea of an uncoercive moral or spiritual authority developed ever-increasing associations with the idea of power, until it became the moral or spiritual justification for the exercise of coercive force, thus stabilizing the idea of authority as a fixed quality of power until the next breakthrough.

We can identify three of these breakthroughs and subsequent stabilizations between the Romans and the present—breakthroughs that reproduced the older idea of spiritual and moral authority successively in the spheres of religion, politics, and society, and stabilizations that turned them into additions to the modern idea of authoritative power.

The medieval Catholic Church *perpetuated* the connection of authority and official government in the law which it took over from the later stages of Roman Empire, but it also *innovated* the spiritual authority of the Scriptures and the Church Fathers over the assenting souls of men. As authorized trustees of this divine tradition the Popes explicitly distinguished their dominion as “authority” from the Kings’ as “power.”<sup>5</sup> The Divine Incarnation which built God’s warrant into the rules of the earthly order, the Divine ordination of the temporary and ecclesiastical offices which governed that order, the Divine creation of the hierarchy of superior to inferior beings which stretched from heaven through the earth as the unquestionable constitution of

<sup>5</sup> Especially in the famous distinction of Pope Gelasius I.



the cosmos—all these positions, which contributed to the power of superiors over their subjects, had their Christian origins in an authoritative conception of God, His Son, and His Word that was to be broadcast to benighted souls the world over and freely received by them. This kind of authority was deemed complementary to both tradition and reason. In early Christianity tradition, as an autonomous consensus of the generations on the meaning of Christ's message for human living, was one of the three roughly equal forms—along with Scripture itself and the episcopal office (including the Roman papacy)—taken by spiritual authority.<sup>6</sup> For St. Augustine authority and reason dovetailed in a consistent process of equivalent parts, whereby authority provided the definitions and the stimulus for the necessary role of reason in the formulation of the rules of faith.

But medieval conditions for propagating a unified faith among disparate peoples and medieval needs for the Church organization in secular life led to the growing association of Christian authorities with ecclesiastical power, to the overlap of ecclesiastical temporal categories, and to intellectual conflicts between the spiritual and the power-full ideas of authority in which the latter won out. The idea of authority was increasingly vested in the papal “power of the keys” which, with the aid of the canon lawyers and papalist theologians, made the definition of tradition a function of this power and in principle excluded reason from the formulation of the rules of faith.<sup>7</sup> By the fourteenth century not only were the papalists applying the pope's “plenitude of power” to temporal as well as church government, but even the Conciliarists, despite the advocacy of corporate tradition in matters of spirit, were, in the writing of Ockham and Marsilius of Padua, deriving moral precept and external organization alike from the command, the will, and the power of both God and the community.

The principal issue of the Reformation, both in the early opposition of the reformers to the contemporary forms of Catholicism and in the subsequent divergence of institutionalized Protestantism from the spirit of the early Reform, should be seen as a conflict not between religious liberty and religious authority, but between the two kinds of religious authority. It began as a restatement of the original kind of religious authority against the later imperious developments of the Catholic church and then itself recapitalated the Catholic evolution. Luther and Calvin climaxed the long line of resistance to the triumph of ecclesiastical power over the direct authority of Scripture, and they led this resistance in the name of a justification by faith and a literal reliance on Scripture which were simple theological terms for the direct and unimpeded flow of God's definitely defined spiritual authority to all kinds of men everywhere. But their apparent victory was shortlived, because their churches, too, quickly took on the usual pattern in the development of authority and grew from the pious spirituality of their origins into alliances

<sup>6</sup> Karl F. Morrison, *Tradition and Authority in the Western Church, 300–1140* (Princeton, 1969), esp. 33.

<sup>7</sup> A. J. MacDonald, *Authority and Reason in the Early Middle Ages* (London, 1933), *passim*; Michael Wilks, *The Problem of Sovereignty in the Later Middle Ages* (Cambridge, 1963), 375–77.

with temporal powers that endowed these powers with the title of authority and ecclesiastical authorities with the instruments of power.

I have dwelt upon the Roman and medieval cases because both the original idea of autonomous, uncoercive authority and the historical process which changed it into a legitimation of coercion were overt in them and because these cases furnish keys to the recognition of both the idea and the process during the modern period, in which they have been obscured by the explicit and continuous association of authority with political power. The identification of authority with legitimate government is indeed, as we have seen, the paradigm of modern authority as such, but the gross association of authority with the right to exercise a public power should not blind us to the two kinds of relationship which have gone into the forging of this association. The production of the modern state between the sixteenth and the middle of the nineteenth centuries was a prime creation of Western culture, and, like the other cultural creations of the West, it was diffused first through its claims to an uncoercive authority before its establishment as a legitimate coercive institution. Although the process in this instance was confused by the circumstance that the cultural product was collective power itself, there remained nonetheless the crucial distinction between the early modern idea that an uncoercive authority was necessary to the very constitution of a political power and the nineteenth-century idea that an authority was simply the legitimate exerciser of a pre-existing political power.

The reliance of the early-modern state upon uncoerced and uncoercible compliance was attested by theories which grounded both the origins and the ultimate purposes of political power in a principle which lay between power and rights and was categorically distinct from both. Once Bodin, Grotius, Hobbes, and others of the dominant natural-law school set aside, as disorderly, the immediate derivation of the ruler's powers from divine ordination and the natural hierarchy and vested the origins of those powers in the consent of the community, they were confronted with the problem of the convertability between the community's right, which was an original power over itself, and the ruler's power, which was a derived power over it. The idea of authority became central to their scheme of origins as the voluntary general authorization of the ruler's function which made his particular powers different from and unaccountable to the corresponding constituent powers of the community. In respect to the ends of the state, analogously, the early-modern theorists grounded political compliance ultimately in a notion of the common good transcending the services which could be demonstrably secured by the sovereign's use of the collective power. The very ambiguity which has so often been noted in them between the compulsory peace and order they emphasized and the ideal moral unity of their final resort testified to the theorists' registration of an attitude toward the benefits of power that required the belief in a transcendent political moral authority to produce conformity even when the benefits of power were not forthcoming. As a principle of origins as well as of ends, authority was then built into the modern service-state as the in-

definite appeal for loyalty over and above the services the state actually renders.

The form that political theory now took was to relate human freedom and authority internally as two sides of the same moral force. Thus, however much they differed in the specifics of their politics both Hobbes and Locke agreed that the supreme authority of the sovereign—in Hobbes the ruler, in Locke the legislature—is an authorization from the community as a whole, whose natural rights become the original authority through the authorization. The moral and spiritual basis of this original authority—that is, the liberty and rights of man—becomes explicit in Rousseau, whose idea of the general will is so mysterious and difficult precisely because it is the point at which men turn their various rights into a single authority, and is so spiritual that it is embodied in no earthly institution.

The method of this early modern political theorizing came from the mathematical mode of reasoning in the contemporary natural philosophy of the seventeenth and eighteenth centuries. Now reason as such—especially deductive reason—has an ambiguous relationship to authority. The compulsive force of a deductive logic, where the conclusion follows necessarily from an axiomatic or previously demonstrated premise, is not authoritarian because it carries its own credentials. The mind does not submit to the rational conclusion but appropriates it fully and freely as its own. Power and freedom can meet in reason, to the exclusion of authority. But reason can be employed in the service of authority when the premises must be taken on trust. In the science and philosophy of the seventeenth and eighteenth centuries such employment, and especially the use of Aristotle for the purpose, was violently rejected in favor of rigorous methods grounded in self-evident premises which made the rational method an exercise of freedom for their subscribers but also made it the method of a new invisible authority for their detractors. There was a precise parallel between the idea of men using their freedom to create an authority which would presumably enforce the goals of their freedom and the method of using self-evident axioms as the basis of a necessary logic which would issue in inescapable conclusions presumably available to but actually not shared by every mind.

So this kind of rational authority, with its apparently compatible relations to liberty and reason, did not last either, but yielded to the same kind of displacement as had previously overtaken its predecessors in law and religion. As the foundation of rational method shifted, during the eighteenth century, from deduction to induction, from axioms presumably self-evident in all minds but actually propagated by an intellectual elite to concrete experience actually verifiable by every man, the older reason was seen as an alien authoritarian imposition and the newer reason, emphasizing tentative conclusions limited to the available evidence, was associated with the freedom of inquiry and lost all connection with the liberal idea of authority. In a parallel way the idea of liberty was detached from the idea of authority, which became attached to the ideas and institutions of conservative power.

The culture of the eighteenth century has long been known for its mixture of rationalism and empiricism. It should hardly be surprising, then, to find that the century was analogously transitional in its modulation of independent political authority, which was generally approved, to authoritative power, which was the target of conflict. From Locke through the physiocrats to Burke, historical revisions have supplied the missing authoritarian dimension to the traditional liberal caricature and a liberal, powerful dimension to the long-standing authoritarian label. But there could be no question about the direction to which the future belonged. From the liberal intellectuals of the Enlightenment to the philosophical radicalism of the young John Stuart Mill and Karl Marx at mid-nineteenth century, empiricism and naturalism in philosophy were commonly associated with a corresponding defense of political freedom against ideas and institutions of authority viewed equally as attributes of the established power. Elaborating on a selective vision of Locke's argument against innate ideas and for the derivation of all knowledge from "experience," the French philosophes—at least in the instance of a bellwether like Diderot—attached human rights to a categorical doctrine of epistemological sensationalism and dissolved political authority into a part that was a function of the community's liberty and a part that was a function of the ruler's power.<sup>8</sup> Around the same time Adam Smith was preparing for the laissez faire of his *Wealth of Nations* by dividing the idea of moral authority in his *Theory of Moral Sentiments* between "our moral faculties . . . within us," represented above all by the conscience of the individual, and "the commands . . . laid upon [man] by infinite wisdom and infinite power," enforced by "the punishment of God, the great avenger of justice."<sup>9</sup> Somewhat later the young Mill, in the same vein, denied the logical independence of the syllogism, subsuming it under a process of induction that started from duly verified particulars. Correlatively he became a political and cultural liberal in whose eyes "government is always either in the hands, or passing into the hands of whatever is the strongest power in society."<sup>10</sup> He grew up to see, moreover, and continued to see recent history as dominated by the conflict between liberty and authority—by which he meant authoritative power. Well might Bertrand Russell sum up this whole tradition with the simple assertion: "The only philosophy that affords a theoretical justification of democracy, and that accords with democracy in its temper of mind, is empiricism"—or, as he preferred to call it, "empiricist Liberalism."<sup>11</sup>

This evolution of the idea of political authority from its constitutive inde-

<sup>8</sup> Locke's grounding of his argument against the proponents of innate ideas could have stood as a literal model of the hostile connection between rationalist and political authoritarianism: "Nor is it a small power it gives one man over another, to have the authority to be the dictator of principles." John Locke, *An Essay Concerning Human Understanding*, ed. A. D. Woodzley (New York, 1964), 87, 89; Denis Diderot, *The Encyclopedia*, ed. Stephen J. Gendzier (New York, 1967), 2-4; Leonard Krieger, *An Essay on the Theory of Enlightened Despotism* (Chicago, 1975), 53.

<sup>9</sup> Adam Smith's *Moral and Political Philosophy*, ed. Herbert W. Schneider (New York, 1970), 160, 178, 192-93, 198-99.

<sup>10</sup> John Stuart Mill, *Autobiography* (London, 1924), 121, 135-37.

<sup>11</sup> *The Basic Writings of Bertrand Russell: 1903-1959*, ed. Robert E. Egnier and Lester E. Denonn (New York, n.d.) 462, 467.

pendence in the early modern period to a function of established public power in the eighteenth and nineteenth centuries, indicated by the liberals of the latter period, was positively confirmed by conservatives who were their contemporaries and by osmotic concessions of liberals themselves in the second half of the nineteenth century. The conservatives' attitude toward authority was in part a reaction against the antiauthoritarian facts of the French revolution and in part a mirror image of the liberal attitude toward authority to which they were responding. In both cases the result was that conservatives in the nineteenth century endorsed the union of authority and power. Certainly this was true of the main pillars of Restoration political thinking—Joseph de Maistre, Adam Müller, and Ludwig von Haller. De Maistre, for example, insisted that authority was intrinsically antithetical to both the reason and the rights of ordinary men and identified it exclusively with the legitimate power devolved by the Divinity upon the rulers of state and society. He endorsed the sentiment: "Power on one side, weakness on the other: this constitutes all the bonds of human society."<sup>12</sup> It should not be surprising, then, to find a recent defense of Hegel against the charge of conservatism couched precisely in terms of his rejection of authority as authoritative power over the community and his polemics against the conservatives who so defined the idea of authority.<sup>13</sup>

A second sign of political authority's development into an adjunct of public power in the modern period can be seen in the reluctant acceptance of such authority after the middle of the nineteenth century even by those liberals and radicals who had originally shunned it because of its association with political power. The growing respect for traditional and intellectual elitism in government on the part of such qualified liberals as Tocqueville and Mill is too well known to bear extended repetition here, but two lesser-known examples of similar development taken by famous radicals who spanned the middle of the nineteenth century may be adduced now. Whereas the youthful Marx had used dialectical reason to deny any possible authoritative intermediary between the natural rights of the human community and the oppressive power of its alienated masters and thought of the coming Communist revolution in terms of removing "fetters," the subsequent exposition of Marxist political theory by the older Engels under the later challenge of anarchist competition asserted the necessary role of authority as the title to raw power in the coming Communist revolution. He defined authority simply and brutally as "the subjection of another's will to our own." And he declared that the socialist party must itself use it, like all victorious revolutionary parites, to "force its will on another part" of the population through "the terror exercised by its weapons."<sup>14</sup>

<sup>12</sup> Joseph de Maistre, *On God and Society*, ed. Elisha Greifer (Chicago, 1959), 6; see also *The Works of Joseph de Maistre*, ed. Jack Lively (New York, 1971), 4-5, 207.

<sup>13</sup> Shlomo Avineri, *Hegel's Theory of the Modern State* (Cambridge, 1972), 47, 180-83.

<sup>14</sup> Karl Marx, *Early Writings*, ed. T. B. Bottomore (New York, 1964), 155-57; Karl Marx and Frederick Engels, *The German Ideology: Parts I and II*, ed. R. Pascal (New York, 1963), pp. 70-71; F. Engels, "Über das Autoritätsprinzip," *Neue Zeit*, 32:1.

But it was in Kierkegaard that the potent character of the authority finally conceded by intellectual radicals of the nineteenth century was most drastically displayed. After a decade (from the mid-thirties to the mid-forties) of writing about the paradoxical immediacy of the relationship between the existent human individual and the absolute God, to the total neglect (and implicit rejection) of any mediating principles like the original idea of authority, Kierkegaard apparently turned about to proclaim the necessity of authority as the remedy for the chronic spiritual illness of his age. But the apparent turnabout was more of a development from than a reversal of his earlier implicit ideas of what authority must be.

Actually it was not the original mediatory but the very different transcendent and potent idea of authority, as “paradoxical” as the fundamental God/man relation with which it was consistent, that he now explicitly espoused. Authority in this sense has nothing to do with genius or service or indeed with any substantive quality of the delivered message that might serve to relate the individuals who are the objects of the authority to its wielder; it has to do only with the unconditioned title to obedience, “which comes from another place.” The only true authority is founded on the “eternal, essential, qualitative difference” between God and man; it is exercised only “when God appoints a particular man to have divine authority”; and it consists, therefore, in the fact of this appointment pure and simple. Nor did Kierkegaard shrink from the awful implications of this kind of authority. The counterpart to the “authority” of the apostle is the “obedience” of the believers, and, although all earthly authority is “a vanishing factor” because of the inevitably egalitarian and interactive dimension in human relationships, the worldly model of true authority can only be the unquestionable authority in the king’s command, “for this analogy demonstrates the compulsory character of an authority” that “prohibits all critical and aesthetical impertinence with regard to form and content.”<sup>15</sup> Analogously with other revisionist individualists of his age, the authority which Kierkegaard finally accepted was very much the same as the authority with which he earlier would have nothing to do.

The last of the completed cycles of authority in Western history was initiated by the creative surge which constructed industrial society and found ideological expression in the theories of society it spawned. As in previous cycles the new creation rejuvenated the older, autonomous idea of authority, attaching it to the independent reality which had just been acknowledged—in this case social relations as such—and for this purpose separating it from the political institutions of power which militated against innovations of this kind. In the words of a twentieth-century theorist who reinterpreted the democratic state to fit it for the society created by the industrial revolution: “Those rules which the state enforces cannot possibly be identical with the moral standard which ought to govern our lives, and the authority of the state cannot be the authority of that standard.”<sup>16</sup> So we find in pioneers of the new science of soci-

<sup>15</sup> Søren Kierkegaard, *On Authority and Revelation*, tr. Walter Lowrie (New York, 1966), 108–17.

<sup>16</sup> A. D. Lindsay, *The Modern Democratic State* (New York, 1943), 1:207–08.



ology—in Spencer, Durkheim, Max Weber, Michels, and Pareto—notions of authority specifically appropriate to social relationships and specifically distinguished from political power.

However separate in principle the early stage of social authority and the later stage of political authority may be, the thinking of the sociologists shows that by the end of the nineteenth century the historical weight of the modern political idea of authority had become so impressive that the usual development from the moral to the political idea of authority was no longer a development from one to the other through time but a simultaneous vacillation between them. The political idea was very much in the picture even for the first generation of sociologists, including Max Weber himself, and what had previously been a clear development now became a confusing mixture. Thus the identification of autonomous social authority in industrial culture raised thorny problems for the sociologists—problems which reflect the fundamental problem of authority in the contemporary world. For the only kinds of distinctive social authorities identified by the sociologists were dead authorities—that is, charismatic individuals, “segmental societies,” and traditional institutions, such as the military and the family, which were registered to be still existent in fact but atavistic in principle, by reference to modern society.<sup>17</sup>

When these sociologists of the early twentieth century undertook to uncover a principle of social authority that was appropriate to contemporary industrial society and yet distinct from coercive political power, the result was what we might call essential ambiguity. Max Weber’s distinctively modern authority—bureaucratic authority—was common to compulsory political and voluntary social institutions alike, and not only did he not decide on the priority of the political or social spheres but he did not even treat it as an issue.<sup>18</sup> Pareto defined elites in terms of general social capacity but then focused his discussion on the authoritative power of the governing section of those elites. Robert Michels demonstrated his “iron law of oligarchy,” which was presumably to denote the authoritative structure of all democratic social organizations, by recourse to the modern party—the precise crossroads of society and politics in our culture.<sup>19</sup> In these theories both the identity and the confusion of the two ideas of authority are complete. Elites are the effects of the natural laws of societies and are therefore acknowledged naturally, without compulsion. But political power is also one of the natural functions of society, and elites are never without it.

In 1936, finally, the Frankfurt Institute for Social Research’s monumental “Studies on Authority and Family” carried the same mutual, albeit anomalous, permeation of the social and the political dimensions of authority over into the empirical sociology and totalitarian conditions of the mature twentieth century. Max Horkheimer’s epitomal essay hearkened back to the autonomous period of authority in early bourgeois culture, when the authority of

<sup>17</sup> For archaic “segmental societies” and their modern residues, see Emile Durkheim, *The Division of Labor in Society*, tr. George Simpson (New York, 1964), 174–81.

<sup>18</sup> Max Weber, *Basic Concepts in Sociology*, tr. H. P. Secher (New York, 1962), 115–18.

<sup>19</sup> Robert Michels, *Political Parties*, tr. Eden and Cedar Paul (New York, 1959), 23–40, 390.

the father in the family and the authority engendered in the society at large by "patriarchal education" were spontaneously related in a purely social interaction, and he contrasted it to the subsequent, contemporary period of bourgeois culture when the authoritarian family was rendered self-contradictory by being both persistent and politicized. Thus on the one hand the hierarchical family remained "indispensable," but on the other its independence was undercut by the state and has become "a problem of mere governmental technique." The only way, indeed, that Horkheimer could make sense of the family as a politicized structure of authoritative power that was buttressed by the inert remnants of an autonomous social authority was by resorting to that last measure of the rationalist's desperation when confronted with confusion and contradiction—the dialectic—attributing authority to a "dialectical whole" composed of "a unity of divergent forces."<sup>20</sup> The formulation may have been idiosyncratic, but the idea was a representative indication of the modern interpenetration of autonomous and compulsory authority.

What was ambiguous in these ideas was the infiltration of coercive powers formerly identified with governing sovereigns into the structures of presumably autonomous social authorities. What was essential about these ambiguities was their reflection of the democratic industrial civilization, with the effect of extending the effective areas both of power and of liberty at the expense of the autonomous authority which has been the traditional buffer between both. The obvious resolutions of the ambiguity have been effected by subsequent developments into totalitarianism and by the anarchism which has been the reflex response to it. Both movements denied the autonomy of authority and drew their respective conclusions from the permeation of all social relations by the political model of coercion.

WE HAVE TWO FINAL QUESTIONS TO ASK. How does our dual inquiry into the two kinds of authority clarify the historical riddles about the nature of authority? And what does this historical clarification of authority contribute to the understanding of our present crisis of authority?

The continuing riddles in the history of authority consist in the opposite answers which men persist in giving to the questions of its relations with other crucial ideas. They have time and again pitted reason against authority, but speak of rational authority. They have insisted upon the limitation of authority by law, but speak of the authority of the law. They have opposed authority in the name of liberty even when the authority is that of an egalitarian majority, and they have opposed authority in the name of equality, insisting that liberty in the form of voluntary compliance is an essential condition of this authority. We can throw light on all these puzzles, if they are interpreted in terms of the different ideas of authority that have in fact been meant. Men have generally assumed that any act or belief commanded by reason is an exercise of freedom. Reason itself can never be an authority, therefore, but an

<sup>20</sup> *Studien über Autorität und Familie* (Paris, 1936), 75–76.

educative authority such as Rousseau's Legislator, who transitionally imposes reason upon willing and capable recipients not yet ready for it, has a rational authority, while the prescriptions of a permanently ordained superior have regularly been deemed antithetical to reason when their authority was exercised through mental or physical compulsion. To law, analogously, authority has been attributed when the validity of the law was grounded in its rational content, and the authority was grounded in men's accessibility to it, but law in this sense has been opposed to authority when the latter was vested in the simple command of a superior. Equality, finally, has obviously been the antithesis of uncoercive authority, and liberty the antithesis of authoritative power.

These two kinds of relationship of authority to other values depend on which of the two main ideas of authority is in question. If we put together now the results of our two historical surveys of these ideas we may conclude that each creative burst of our culture has been accompanied by the elevation of authorities whose superiority is freely accepted by dint of their rationality and legality, but that our modern idea of authority as a title to domination however exercised is a teleological idea derived from the use of force, the hostility to reason, the superiority to law, and the opposition to liberalization which these authorities have cumulatively appropriated.

Where, then, do we stand with our crisis of authority after our historical inquiry? We may project the following probabilities.

First, the decline of authority—seen as an independent moral and intellectual way of getting people to do or agree to something before they can understand adequate reasons for freely doing or agreeing to it and without using compulsion or duress to make them do or agree to it—is historically deep-seated, and the idea of this authority is probably doomed in every one of the shapes familiar to us. This is not to say that the institutions of such authority, like the family and the school, will disappear at once, for social inertia remains a powerful force. But they will survive as facts, without the support of a sustaining idea. Like government, with which they have been connected in recent history, they become more and more institutions dispensing an ill-assorted mixture of welfare, barter, and coercion.

Secondly, independent moral authority has declined not only in favor of the equality which recognizes no superiors and requires men to act with complete freedom, on adequate reasons and for adequate returns, but also in favor of the political power for which it has furnished an ever more tenuous justification. But even if, then, the demise of authority as we have known it is deemed a good thing—and I, for one, welcome the demise of any hindrance to rational choice and free decision—we must recognize that this demise will leave freedom-seeking individuals directly confronting institutionalized power, separated only by the façade of responsible authority and actually bereft of their older authentic buffer.

Thirdly, the imminent demise of all the familiar kinds of independent authority does not mean the permanent disappearance of all possible kinds of

independent authority. It has, as we have seen, recurred unpredictably and in unpredictable forms with every fundamental cultural innovation, and consequently its future recurrence at a time and in a shape we cannot foresee is also probable. Not only probable but, as a strictly temporary thing, desirable, since in its recurrent transitional role authority without power has persuaded men to accept innovations on trust and in a voluntary way leading ultimately to their full understanding and free commitment.

If, finally, we are to hazard a guess about the next arena of cultural emphasis and its accompanying hierarchy of acceptable values, we should probably look to the individual psyche, for there is contemporary evidence that the growing loss of belief in any valid authority *between* individuals is being compensated by a growing belief in a valid authority among the drives *within* the individual. The large audience for neo-Freudians like Norman Brown, Erikson, and Marcuse; the renewed attention to Jung; the vogue of existential psychology—these are a few indices of the new focus on the reordering of psychic priorities and on the organization, within the personality, of a unity for action which has always been the function of authority. Since, in this contemporary view, the centuries of social authority have wound up only with the production of authoritarian personalities and one-dimensional individuals in the very image of the coercive society, why not reverse the process and see whether self-integrated individuals can produce a rational social authority in *their* own image? The prospects are that, like other forms of innovative authority, the psychologically authoritative individual will indeed lead those not so well integrated as he without compulsion—for a while.

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## Rules of Inheritance and Strategies of Mobility in Prerevolutionary France

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RALPH E. GIESEY

IN THE THIRD CHAPTER OF *De la démocratie en Amérique*, Alexis de Tocqueville undertook to explain the profound influence of the laws of inheritance. "I am surprised," he said, "that ancient and modern political writers have not attributed a greater influence on the conduct of human affairs to laws dealing with successions. True, these laws belong to civil affairs, but nevertheless they belong at the forefront of political institutions because they exercise an incredible influence upon the social status of a people, of which political laws are but a reflection." Tocqueville's aim was to show how the principle of equal division among heirs had democratized America: "The last trace of ranks and hereditary distinctions is destroyed; the law of successions has hastened the process of leveling everywhere."<sup>1</sup>

It would be interesting to know how Tocqueville would have handled the question of the old French laws of inheritance had he chosen to discuss them when he wrote *L'Ancien Régime et la Révolution* twenty years later. In the most populous and prosperous regions of France, the northern two-thirds where customary laws (*coutumiers*) prevailed, Tocqueville would have found that all but the nobility lived under laws which called for the same kind of equal division among heirs which he extolled in early nineteenth-century America. How would he have explained the rise of great bourgeois and office-holding dynasties in the two centuries before the Revolution? A charitable answer is to excuse Tocqueville from entering the realm of *coutumier* laws of succession because it is an exceedingly dense legal thicket. The initial impediment is the variety of ways in which like points of law were expressed in the scores of *coutumiers* as they were redacted in the sixteenth century. The entanglement

This is a revised version of a paper given at the XIV International Congress of Historical Sciences (San Francisco, 1975) under the title "National Stability and the Hereditary Transmission of Political and Economic Power." The National Endowment for the Humanities supported my research. The constructive criticisms of many people have been embodied in the present version; unable to mention all, I cite as token one to whom I feel especially indebted: Charles K. Warner.

<sup>1</sup> *Oeuvres*, I (Paris, 1951): 46-47, 50. Lord Bryce was unable to confirm this vision of equality when he visited America in 1889 (cf. Richard Parker, *The Myth of the Middle Class* [New York, 1972], 187), and indeed its existence in Tocqueville's own time is now relegated to the realm of fancy: see Edward Pessen, *Riches, Class, and Power before the Civil War* (Lexington, Mass., 1973), 1 and *passim*. As for our own time, the works of Ferdinand Lundberg show how family fortunes maintain themselves through many generations.

grows greater as one penetrates the court decisions and juristic writings of the two and a half centuries before 1789. Recent legal scholarship has clarified matters on the level of *dogmatique*, but the utility of this information for social historians remains limited because of our poor understanding of *pratique*, i.e., how family fortunes, especially middle-sized and smaller ones, originated and were transmitted intact over many generations.

The kind of quantitative information so coveted by modern historians may never be acquired for these matters, but the problem is too important to be set aside simply because it is not now amenable to statistical analysis. In this essay, therefore, I propose to set forth the general principles of succession to "lineage property" (*propres* in French), to ponder the role of two unusual elements in such property (that is, annuities and offices), and finally to speculate upon how all these things worked directly or indirectly to promote stability within the French nation.

MOST LEGAL SYSTEMS DISTINGUISH between personal property (*meubles*) and real property (*immeubles*, "immovables") for such purposes as contract and obligation, but in France the different *coutumiers* also divided real property (originally only land and houses) into separate categories for purposes of inheritance: *acquêts* and *propres*. The former were immovables a person had acquired (hence *acquêts*) by the fruits of his or her own labor. Individuals had the right to dispose of them freely by gifts during their lifetime or by testament upon their death. *Propres*, on the other hand, were immovables which individuals got from their parents and were required to save for their own children. (Also, *acquêts* became *propres* when they were transmitted to children.) In brief, *propres* were considered to belong corporately to the successive generations of the lineage, so that the individual's right to dispose of them by gifts or in testaments was limited.<sup>2</sup>

The concept of *propres* appeared in medieval laws of commoner (*roturier*) succession separately from the laws of noble succession. Originally the term "*propres*" was a way to identify the family origin of property, in the case, for example, of property with which a bride and groom were endowed. If the marriage produced no offspring, those *propres* (or equivalents) with which the couple had been dowered had to be returned to their respective parents: *paterna paternis, materna maternis* was the primary rule of lineage property. If the new family reproduced itself for a few generations but then failed, the respective paternal and maternal *propres* still had to find their way back to the lineage whence they originated.

<sup>2</sup> The fundamental manual is Philippe de Renusson, *Traité des propres*, 1681 and often republished. The *thèse de droit* by Marcelle Durupt, *Évolution de la notion de propre dans la coutume de Paris du XVI<sup>e</sup> au XVIII<sup>e</sup> siècle* (Poitiers, 1938) is systematic but lacks good historical development. A fine introduction to the subject of *propres* is given in the sixty-five-page essay by Charles Lefebvre, *Les fortunes anciennes au point de vue juridique* (Paris, 1912). A fair French textbook introduction to the subject by Jean Brissaud became a disaster at the hands of an English translator who called *propres* "personal possessions" during the old regime (as if the same as *biens propres* in modern French law): see *A History of French Private Law* (Boston, 1912), 273–78, 642–50. To my knowledge, the only general historian who has given much attention to *propres* recently is Roland Mousnier, *Les institutions de la France sous la monarchie absolue* 1 (Paris, 1974): 47–69, but his treatment has flaws (see below, note 18).



Thus defined, *propres* seem to be property that regressed to ancestral lines. In practice, however, they constituted a kind of trust for succeeding generations of the new family. Children eventually acquired *propres* not only from their grandparents (*propres anciens*) but also from their parents, whose *acquêts* became *propres* when they passed to heirs (*propres naissants*). This established an unusual relationship between the individual and the *propres* in the estate. It has been termed “lifetime ownership” (*propriété viagère*): the use of the property but not the right to dispose of it to anyone but heirs.<sup>3</sup>

Since all immovables were susceptible to becoming *propres* when they devolved by succession, the juridical definition of immovables proved to be the critical factor in the historical importance of the system of lineage property. Originally including only such “immovables by nature” as land and houses, the definition of *immeubles* was extended in the later Middle Ages to include *rentes foncières* (perpetual annuities received in exchange for the alienation of land or a house). The person who acquired the property, the “debi-renter” (*debirentier*), became the true proprietor in law, while the former owner who had alienated it became the “credi-renter” (*crédirentier*—the true *rentier* in the old law) and was endowed with a perpetual annual income based upon a percentage of the worth of the property alienated. Since a given piece of land or a house provided the security for the transaction, the perpetual *rente* given in exchange was deemed an “immovable by fiction” because of its contractual relationship with an “immovable by nature.” The annual *rente* was received in the form of money, the purest of *meubles*, but it was classified in law as an *immeuble*: an *acquêt* in the estate of the first credi-renter, a *propre* in the estate of his heirs.<sup>4</sup>

The sixteenth and seventeenth centuries witnessed two further extensions of the definition of immovables that consequently transformed the function of *propres* in successions: first, *rentes constituées* (perpetual annuities in exchange for a capital sum), then royal offices held by families on an indefinite basis.

A *rente constituée* differed radically from a *rente foncière* in the estate of the debi-renter: in the *rente constituée* he received a sum of money, in the *rente foncière* a piece of real property. In the estate of the credi-renter, however, there was no difference between the two kinds of *rentes*: they both brought the same kind of annual income in perpetuity to him and his heirs. *Rentes foncières* earlier had been deemed immovables by fiction because of their contractual relationship with immovables by nature; now, *rentes constituées* were called immovables by fiction because they served the credi-renter in the same way as the earlier immovable by fiction, the *rente foncière*. In effect, the first immovable by fiction bred its own kind.<sup>5</sup>

<sup>3</sup> Edouard Lambert, *De l'exhérédation et les legs faits au profit des héritiers présomptifs* (Paris, 1895), 432.

<sup>4</sup> Charles Lefebvre, *Observations sur les rentes perpétuelles dans l'ancien droit français* (Paris, 1914), provides a lucid seventy-six-page survey of the legal aspects of *rentes*, and the complexities of the subject during its formative period is dealt with by Bernard Schnapper, *Les rentes au XVI<sup>e</sup> siècle* (Paris, 1957).

<sup>5</sup> Lefebvre, *Rentes*, 54–76, but more authoritatively, René Filhol, *Le premier président Cristofle de Thou* (Paris, 1937), 249–90. By fixing *rentes constituées* at a rate below what was considered usurious (see below, note 30) and making them terminable only by the debi-renter's returning the capital (or by a cumbersome legal process of forfeiture for nonpayment of the *rente*), this form of perpetual loan became nonusurious in terms of canon law and thus the principal method of lending—although in law actually an alienation—during the *ancien régime*.

This process was repeated in the seventeenth century when *offices* came to be classified as immovables. Again juristic philosophy defined the thing in law according to its temporal and not its physical aspect. The *pauvette* in 1604 had the effect of endowing offices that had regularly become venal during the previous century with the quality of being transmissible by inheritance. Offices therefore provided the series of successive holders with the same kind of regular revenue as did *rentes*, and so, like *rentes*, offices became fictive immovables and consequently *propres* in family successions.<sup>6</sup>

We shall return later to the political and economic consequences of offices' and *rentes*' becoming family *propres*. These developments have been introduced here only to show the vast scope of *propres* in customary laws: land and houses actually owned; *rentes foncières* established on real property which had been alienated; *rentes constituées* established on capital which had been alienated; and royal offices. According to the *coutumiers*, all these things were supposed to be conserved, and if possible augmented, over the generations. In the short run, legal strictures required conservative handling of *propres*, but this also enhanced the possibility of slow but steady growth over the generations.

The laws governing *propres* were diverse and complex. One of the main categories was protection of a wife's *propres* from the depredations of her husband, who in general had control over family property. "Husbands get up three times during the night in order to sell their wives' property," ran a medieval proverb; *ancien régime* jurists were fond of quoting it when explaining later laws preventing such chicanery.<sup>7</sup> We must bypass this, however, as well as such things as the *retrait lignager*, which allowed heirs to recover *propres* alienated by their parents.<sup>8</sup> Instead, we shall concentrate on how non-noble families handled the most dangerous threat to their heritages: excessive division among a large number of heirs.

Legal systems which make no distinction between the kinds of property involved in succession and which allow great liberty of testamentary disposition to the deceased (Roman law and English law, for example) pose no problem to advantaging one heir among many in order to keep the patrimony intact. But in French *coutumier* succession, where *propres* were distinguished from *meubles* and *acquêts* (both of which could be disposed of freely), *propres* were supposed to be divided equally among heirs without distinction of sex, and, accordingly, testamentary liberty was supposed to be limited.<sup>9</sup> The two "supposed to be" conditions in that sentence intimate how the *coutumiers* were twisted in order to keep family heritages intact over the generations.

The *Coutume de Paris*, which influenced other *coutumiers*, classified four-fifths of the *propres* as the *réserve héréditaire*, which should be divided equally.<sup>10</sup> There

<sup>6</sup> Renusson, *Propres*, V, iv (ed. 1714: 455-514); Durupt, *Propres*, 18-26; also François Olivier-Martin, *Histoire de la Coutume . . . de Paris*, 1 (Paris, 1922): 210.

<sup>7</sup> Antoine Loisel, *Institutes coutumières* [1607], I, ii, 14 (ed. Paris, 1846, 1:152).

<sup>8</sup> Louis Falletti, *Le retrait lignager en droit coutumier français* (Paris, 1923), is the standard work.

<sup>9</sup> Experts will know that some *coutumiers* made *acquêts* subject to lineage rights and that in others males were advantaged over females; exceptions like these will not be noted regularly by me, since I am only concerned here with usual conditions.

<sup>10</sup> Jean de Laplanche, *La réserve coutumière dans l'ancien droit français* (Paris, 1925), 374-412; Gustave Boissonade, *Histoire de la réserve héréditaire et de son influence morale et économique* (Paris, 1873), 252-72.

would be no problem if only one son and one daughter survived to maturity: the daughter would be dowered as handsomely as possible when she married—let us say, one-quarter of the family's *propres*—and then renounce her rights of succession, so that the bulk of the heritage would pass to the son during the parents' lifetime or upon their death. The issue became acute when there were many children or none at all. If many, one of them had to be advantaged if the family heritage were to be kept sufficiently unified to maintain its potential for growth; if none, the choice lay between allowing intestate division of the estate among the couple's parents and siblings, augmenting all collateral lines' heritages equally, or advantaging one or more of them.<sup>11</sup>

If there were many children, the prerequisite for escaping the danger of excessive partition was to avoid letting the *propres* fall under the egalitarian rules of intestate succession. There were two ways to accomplish this: to dispose of the *propres* during lifetime by gifts, advantaging one heir in the process, or to convert the bulk of the estate from *propres* to *acquêts* or *meubles*, which could be disposed of freely by testament. Modern legal scholars are divided as to which of these two methods was preferred to avoid "*amorcellement des héritages*." We know that both were used, but until a fuller investigation of *pratique* is made by historians, we shall have to remain content with the possibilities known to exist within the *dogmatique*.

Lifetime advantaging of one heir usually took the form of a heavy marriage endowment of one child, usually of the eldest son (*institution contractuelle*) or, in the absence of children, of lifetime gifts (*donations entre vifs*)<sup>12</sup> to a selected collateral heir. The key to the success of this system depended on the legal requirement (which varied among the *coutumiers*) that when the parents died an advantaged heir had to report back (*rapport*) the lineage property he or she had obtained earlier and have it measured against what an equal share would have been if all the parental *propres* had devolved intestate. The more progressive *coutumiers* made reporting back optional, so that the advantage given at the time of marriage could be preserved. The child then simply renounced rights of inheritance and kept the lineage property obtained from the parents earlier.<sup>13</sup> The flaw in this system, from the parents' point of view, was that dowering of children at marriage and other forms of donations were irrevocable: "one cannot both give and hold back a donation" was the rule. The longer the parents lived, the more inclined they would be to make lifetime settlements, but longevity was by no means common.

<sup>11</sup> I am assuming the situation where advantaging was not accomplished totally by putting daughters and cadet sons into the religious life or otherwise forcing a lifetime of celibacy upon them; everyone knows about this, but its frequency is arguable.

<sup>12</sup> *Institutions* and *donations* are neatly summarized by Charles Lefebvre, *Successions*, 1: 149–65, 202–22. Francis Garrisson in "Biens individuels et biens familiaux," *Travaux et recherches de l'Institut de droit comparé de l'Université de Paris*, 22 (1963): 27–39, an article unfortunately not documented, holds that *institution contractuelle* was the most important device to advantage one child.

<sup>13</sup> Jean Yver, *Égalité entre héritiers et exclusion des enfants dotés* (Paris, 1966), covers the whole problem, comparing *coutumier* groups with each other and with Roman law. More recently Xavier Martin, *Le principe d'égalité dans les successions roturières en Anjou et dans le Maine* (Paris, 1972), has dealt with the area of strictest *rapport*, where advantaging one child was difficult (cf. 143, 151–53, 159), but I would like to have more evidence of eighteenth-century practice before conceding that *manceau-angevin* "egalitarianism" remained pure until the end.

The weight of modern legal scholarly opinion seems to fall upon the use of testamentary tricks rather than *inter vivos* actions as the major device used to evade the danger of dismembering family fortunes.<sup>14</sup> The problem was to overcome the rule of *coutumier* succession which declared that “no one can be an heir and a legatee at the same time.” Assume the example of the *Coutume de Paris*, where four-fifths of the *propres* constituted the *réserve héréditaire* and had to be divided equally: the testator was free to bequeath one-fifth, but not to someone who also shared in the *réserve* as an heir. According to customary law, legacies were supposed to be nominal (pious bequests to the church and the like), while the law itself declared who should inherit the major portion of the lineage property. The Roman law worked the other way: the head of the family instituted one principal heir, and then in his testament charged that heir by codicils to distribute legacies to others.<sup>15</sup>

Put in a nutshell, the practitioners of customary law developed ways to make their law operate as if it were the Roman law turned upside down. In Roman law the instituted heir was at the top, the legatees in codicils below; in customary laws, the general legacy (*legs universel*) was invested in one person at the top, while the heirs were left to distribute the *réserve*, such as it was. The beneficiary of the *legs universel* was, however, one of the natural heirs who renounced his or her heritage in order to accept the legacy, thereby avoiding incompatibility of being heir and legatee at the same time.<sup>16</sup> To make this efficient, the quantity of *propres* in the heritage had to be reduced so as to minimize the *réserve* to be divided among the remaining heirs. The law winked at such doings as long as the *immeubles* went to an heir of the lineage.<sup>17</sup>

Commoners who had considerable property, many children, and a smart lawyer could thus keep the family heritage intact just as easily as nobles, whose eldest sons were entitled to two-thirds or more of the property by law.<sup>18</sup> If the younger children of commoners had had to rely upon the *réserve héréditaire*, they could have found themselves destitute. To protect them in an absolute way, the customary law borrowed the principle of the *légitime* from Roman law. In customary law usage, the *légitime* guaranteed each child one-half of what he or she would have gotten by intestate succession. This

<sup>14</sup> Above all Lambert, *L'exhérédation*, 390–459, and recently R. Besnier, “Le cumul des qualités d'héritier et la législation dans les coutumes de préciput,” in *Études Dumas* (= *Annales de la faculté de droit d'Aix*, number 43: Aix-en-Provence, 1950), 25–35.

<sup>15</sup> A useful summary of testaments in the north and the south, and a comparison between them, is found in Gabriel Lepointe, *Droit romain et ancien droit français. Régimes matrimoniaux, libéralités, successions* (Paris, 1958), 305–37, 464–68.

<sup>16</sup> Lambert, *L'exhérédation*, 408–12; Charles Lefebvre, *L'ancien droit des successions* (Paris, 1912), 1: 193–201.

<sup>17</sup> Reclassifying, or declassifying, *propres* was done in several ways, mainly through sale or exchange for another *immeuble* which became an *acquêt* and would remain such if passed to a legatee rather than an heir. Thus the use of legacy blocked the development of new *propres*. Lambert, *L'exhérédation*, 419–32, shows how the old *acquêt/propres* distinction parallels the new legatee/heir status, *acquêts* becoming more prevalent as legacy became the major form of succession.

<sup>18</sup> See Lefebvre, *Successions*, 1: 122–47. The tendency of legal historians to treat *propres* as if part of noble succession has been criticized quite correctly by Garrisson (above, note 12), 36. Such is the main flaw of Mousnier's recent exposition of the subject (above, note 2): save in a few places, *droit d'aînesse* applied to *roturiers* only if they happened to possess a seigneurial fief, and entail (*substitution*) was not allowed to (although it was sometimes practiced by) *roturiers* before 1747.

included not only *propres* but also the parents' *acquêts* and all lifetime gifts of immovables they had made. This set an absolute limit on the possibilities of advantaging one child, for, assuming a *réserve* of four-fifths for intestate succession, the *légitime* limited the fraction of advantaging to three-quarters if there were two children, two-thirds if three, five-eighths if four, etc., but never less than one-half to the advantaged one. It may seem ironic that the vaunted *coutumier* principle of equality among heirs was guaranteed ultimately by the application of Roman law principles, but in historical actuality this confluence of Roman and customary laws of succession is characteristic of the whole movement of French jurisprudence during the *ancien régime*.<sup>19</sup>

By the end of the eighteenth century, jurists agreed that Roman law's instituted heir and customary law's general legacy were equally valid testamentary devices to preserve a patrimony intact—each, in fact, as effective in this respect as the guaranteed primogenitary succession of the noble order.<sup>20</sup> Indeed, during the revolution one of the leaders of the Committee on the Constitution, the jurist Merlin, called the system of *propres* “one of the most remarkable and clearly enunciated consequences”<sup>21</sup> of the feudal system. His history was faulty, as was pointed out from the floor then and has been noted in scholarly works since, but he was not wrong in his apprehension that such equalizing elements of succession to *propres* as the *réserve héréditaire* had become part of “a ridiculous assortment of contradictory laws and customs which work only to reproduce the traces of feudal government in a truly political association.”<sup>22</sup> By “feudalism in the modern polity” Merlin meant the nurturing of great families by the system of lineage property, manipulated as necessary to advantage one heir. Historically speaking, the concept of *propres* was born and grew up in the milieu of *roturier* succession; sociologically comprehended, it matured as the legal handmaiden of the newly ennobled and the great bourgeois dynasties of the *ancien régime*.

One of the most perspicacious of modern legal historians, Edmond Meynial, likened *propres* to “perpetual entails,” pieces of lineage property that traveled through the generations of heirs undiminished, in perpetuity.<sup>23</sup> This forward-looking aspect of *propres* in early modern jurisprudence must always be kept in mind. It represented a basic shift away from the original function of *propres* (that is, to identify the genearchal origin of property), a shift that opened the way for commoners' fortunes to bloom and flourish over the

<sup>19</sup> Eusèbe de Laurière, *Les Institutions et substitutions contractuelles*, 3: 35–37 (ed. Paris, 1715, 2: 167–73); Boissonade, *Réserve héréditaire*, 272–311; Lambert, *L'exhérédation*, 60–65; Yver, *Égalité entre héritiers*, 70–73.

<sup>20</sup> Explicit evidence of this is often cited from a work I have not seen, A.-F. Nicolas Levasseur, *De la réunion des qualités de légataire et d'héritier* (Paris, 1790); see Besnier (above, note 14), 30, and Lambert, *L'exhérédation*, 411.

<sup>21</sup> J. Mavidal and E. Laurent, eds., *Archives Parlementaires* (Paris, 1879–), 20: 602 [21 Nov. 1790]. The revolution's abolition of *propres* is traced fully by André Dejace, *Les règles de la dévolution successorale sous la Révolution (1789–1794)* (Paris, 1957), 56–70.

<sup>22</sup> *Archives Parlementaires*, 11: 688–89 (25 Feb. 1790).

<sup>23</sup> “Remarques sur les traits originaux de l'ancien droit privé français,” *Tijdschrift voor rechtsgeschiedenis*, 4 (1923): 406–07; see also Meynial's very pertinent essay, “Remarques sur . . . la succession aux propres,” *Revue générale de droit*, 27 (1923): 326–51. Renusson, *Propres*, III, i, 1–4 (ed. 1714: 239–41), shows contemporary awareness of how future generations were being served.



centuries. This effect can be seen by considering the function in family fortunes and family mentalities of the two uniquely French institutions that were *propres* by fiction and comprised the major part of lineage property: *rentes perpétuelles* and heritable offices.

GEORGE V. TAYLOR HAS PROVIDED a comprehensive picture of "capitalism" in prerevolutionary France, distinguishing four types. Three are "preindustrial" or "premodern" in that they anticipate later forms of commercial or industrial enterprise; the fourth is "noncapitalist wealth," which suggests its nonprogressive nature by its very title. The fourth type, however, is by far the most important element in the economy of the *ancien régime*, for Taylor estimates that this proprietary wealth (the term he quickly adopts in lieu of the awkward expression "noncapitalist wealth") comprised eighty percent of all private wealth in France as compared to only twenty percent for the three protocapitalistic forms. Proprietary wealth provided low return, but involved low risk; and it was very, very stable.<sup>24</sup>

Taylor's "proprietary wealth" consists of three things: land, *rentes*, and offices, which are precisely the three elements that comprise *propres* in the customary laws of succession. If we were to substitute "lineage property" for "proprietary wealth" throughout Taylor's essay, which would be quite valid in juridical terms, we would get a vivid image—the kind Tocqueville would have appreciated—of the role of inheritance in the making of great fortunes during the *ancien régime*.

Since wealth invested in land, *rentes*, and offices was immobilized, the effect upon the nation's capitalistic development was stifling. The preference shown by wealthy families for this kind of investment is the main source of the plaintive remarks uttered so often about the passive character of French capitalism. That by Ernest Labrousse is an example: "Capitalism without power, . . . without a sure structure, . . . without a sure way of reckoning, . . . without the entrepreneur's risking an unsure bet."<sup>25</sup> But from the point of view of the proprietor—the term "proprietary capitalist" comes to the tip of one's tongue—that type of wealth had many advantages. "The returns it yielded were modest," Taylor points out, "but they were fairly constant and varied little from year to year. They were realized not by entrepreneurial effort, which was degrading, but by mere ownership and the passage of calendar intervals. . . . Combined into endowments yielding assured revenues, carefully managed, they could be made to support a family indefinitely in a genteel style of living."<sup>26</sup> This conduct also frequently arouses the disdain of liberal historians, who score *vivre bourgeoisement* as much as *vivre noblement*,

<sup>24</sup> George V. Taylor, "Noncapitalist Wealth and the Origins of the French Revolution," *American Historical Review*, 72 (1967): 469–96 (486–87 for the percentages mentioned); the other three types of capitalism are dealt with in Taylor's "Types of Capitalism in 18th-Century France," *English Historical Review*, 79 (1964): 478–97.

<sup>25</sup> *Histoire économique et sociale de la France*, 2 (Paris, 1970): 703.

<sup>26</sup> Taylor, "Noncapitalist Wealth," 471, 472–73.



the former because it was pusillanimous in facing the dawn of industrial capitalism, the latter because it was prideful despite its apparent social dysfunction.

Taylor's calculations of French private wealth in 1787 list in order of importance: land being exploited (80 percent); houses rented (10 percent); governmental perpetual and lifetime *rentes* (4 percent); and venal offices (2.5 percent). Under the category "Investments in Private *rentes* and Loans" Taylor was forced to enter "[?]" because there were no statistics available.<sup>27</sup> But France's leading jurist of the day, Pothier, reported the following in the 1760s: "*Rentes constituées* are the most common form of *rentes*, and there are many more of them than there are of the other kinds. . . . *Rentes constituées* compose today the largest part of the patrimony, often all the patrimony, of a great number of families."<sup>28</sup> This statement alone is sufficient to make us doubt that we will ever be sure of the overall dimensions of French private wealth (or be able to discern any change in its shape) in the last pre-revolutionary decades until we have reasonable statistical knowledge about *rentes constituées*.<sup>29</sup>

*Rentes constituées* became prevalent because most families—especially the non-noble, upwardly mobile ones—had little land and few houses which they could (or would choose to) alienate in exchange for *rentes foncières*, whereas the fluid capital that had to be given in exchange for a perpetual annuity was constantly being generated by all kinds of money income. *Rentes constituées* were the quickest way to convert these sums into perpetual lineage property. One simply had to find a credit-worthy family in need of capital. When that capital was converted into a *rente* in the credi-renter's estate, it changed instantly from a *meuble* to an *immeuble*—first as an *acquêt*, then as a *propre* when it passed to an heir.

The initial advantages of *rentes constituées* are evident. It is more difficult to understand why credi-renters clung to them when debi-renters threatened to repurchase the *rente*. Rather than recover their capital, credi-renters renegotiated the contracts at a lower rate of return to themselves. The market rate of *rentes constituées* regularly ran below the legal rate fixed by the state. The steady reduction in the stipulated legal rate, from 8 percent in the 1550s down to 4.5 percent from 1665 onwards, was not enacted to protect debi-renters from paying excessive *rentes* but to recognize the ever lower rates credi-renters were willing to accept.<sup>30</sup> As early as the mid-1600s one can discern among "capital-

<sup>27</sup> *Ibid.*, 486, n. 68.

<sup>28</sup> *Traité du contrat de constitution de rentes*, § 166 (ed. *Oeuvres*, 3 [Paris, 1861–: 498] for the first half of this quotation; the second half is given by Lefebvre, *Rentes perpétuelles*, 56–57, apparently from the same work.

<sup>29</sup> Sampling notarial records for selected places and years would yield the size and number of new *rentes constituées*, but this would have to be reduced by the number of old ones terminated. Since terminations were not usually notarized, the information would have to be derived from average length revealed by such documents as multi-generational family household accounts. Unlike royal and provincial *rentes constituées* (such as those on the Hôtel de Ville) which functioned as perpetual bonds because governments rarely had the means to repurchase them, *rentes constituées* between private parties frequently functioned more like term loans. Further, see below the text relative to notes 53 and 58.

<sup>30</sup> See Pierre Goubert, *Beauvais et le Beauvaisis de 1600 à 1730* (Paris, 1960), 538–40, for these facts regarding credi-renters' psychology and the rates of *rentes*.

ists" a kind of thirst for *rentes constituées*—an *emphytéosomanie* if you will—complementing *archomanie*, the mad passion for offices that Loyseau described just after 1600.<sup>31</sup>

If *rentes constituées* were unsuitable to an individual's wish to get rich quickly, they were well suited to a family's hope of getting rich eventually. Imagine the following model: if the revenue from a given *rente* founded at five percent were to be saved for five years and invested in a new *rente*, and the revenue from the old and new *rentes* were saved again for five years and reinvested, this kind of compounding every five years for one hundred years would multiply the value of the original *rente* eighty-five-fold. In three generations, that is, less than 12,000 *livres* could be compounded into 1,000,000 *livres*—the threshold figure for the achievement of noble status. And *rentes*, it must be recalled—as much as they may seem now to have been long-term loans, a soft form of usury—were regarded then as a "noble" form of wealth, not derogating to noble status as were most forms of commercial investment. In terms of social respectability, above all for families entertaining hopes of eventual noble status, *rentes constituées* were a precious resource, the easiest way to cleanse wealth of commercial stain and to lock it securely and honorably into the family heritage.<sup>32</sup>

The long-term effectiveness of compounding *rentes* was subject, of course, to the fiscal risks of devaluation and inflation. Since we are well enough informed about all three factors involved, i.e., rates of *rentes*, monetary alterations, and price indices, it would seem possible to chart the vicissitudes of *rentiers* during any desired span of years during the *ancien régime*. The overall trend of the eighteenth century has already been reckoned. The value of the *livre* was stable from 1727 onwards, and there was general prosperity during the century. The fruits, however, were not equally shared. Prices rose sixty percent, but the income of *rentiers* rose one hundred percent while that of peasants only fifty percent. So, it has been calculated, some thousands of *rentiers* reaped three-quarters of the gains; millions of peasants, only one-quarter. By this time most *rentes* were in the form of *rentes constituées*, money loans in effect, commonly held against peasants forced to indebt themselves at least for short terms when crises occurred.<sup>33</sup>

The unequal rewards of prosperity during the eighteenth century suggest that the real profiteers were those families that had arrived at a level of fortune where a good portion of their wealth could be reinvested. We may some day be able to establish the normal take-off point of family capitalistic enterprise, at which a significant portion of the heritage could be devoted to the kind of constant compounding of perpetual annuities that would allow the building of great fortunes. To establish that point will require many more family histories describing the accumulation of fortunes below 1,000,000 *livres*, the level at which, according to Chaussinand-Nogaret, a family became noble-worthy.<sup>34</sup>

<sup>31</sup> See below, note 44.

<sup>32</sup> The forthcoming work by Robert Forster on the Depont family of La Rochelle during the eighteenth century shows a compounding of fortune and upward mobility which matches many of my hypotheses.

<sup>33</sup> Pierre Goubert, *L'ancien régime. 2: Les pouvoirs* (Paris, 1974), 205–06. See also his contribution in the work cited above, n. 25, 538–40, for these facts regarding credi-renters' psychology and the rate of *rentes*.

<sup>34</sup> Guy Chaussinand-Nogaret, "Capital et structure sociale sous l'Ancien Régime," *Annales (E.S.C.)*, 25

With this information we may be able to detect stages in the development of great wealth through the conservative handling of lineage property.

Successful compounding of a family heritage required that each generation sacrifice something of its own well-being for the sake of future generations. While resembling lawful entail in some ways, *propres* differs in others. Entail guarantees the integrity of the family heritage by placing it beyond the control of any one generation, whereas *propres* reckons that each generation will play an active role in the family's rise to wealth. Many generations, therefore, had to live lives of delayed gratification in order to assure the lasting felicity of descendants far in the future. Such a mentality of family life is hardly known anymore, at least in affluent nations. If we were to try to account for this difference between then and now, one factor predominates: the role of the state in assuring citizens' livelihood.

Today nations commit themselves to maintaining a minimum standard of living, yet sometimes tens of thousands still die of starvation that the state cannot prevent. Those who die have no inherited means to fall back upon in such crises. Not all those born poor will die in this way, but none of those born rich will do so. This is the situation that prevailed periodically in France at least until near the end of the *ancien régime*. "Caring for one's own" meant transmitting a heritage that would make certain that those of one's blood would escape starvation and likely survive the ravages of war and pestilence. In that age the state could not even promise survival, let alone an adequate livelihood, to its subjects. A reasonable family heritage was a life-and-death proposition, an "*autodéfense familiale*"<sup>35</sup> hard to imagine in modern, affluent societies. Apologists for perpetuating family fortunes generally stressed the motive of family glory, but the basic motive was simply that of survival.

This state of affairs was appropriate to the "long-run fatalism" that Rostow finds to be typical of traditional society, but it was not descriptive of the French economic mentality at large before the Revolution.<sup>36</sup> Indeed, the safely affluent could enjoy a sense of optimism in their ultraconservative handling of proprietary wealth. The coin of destiny etched with the grim reaper on one side could be blazoned with noble arms on the other. Nobility was a juridically separate order in France, and it was open to those who acquired vast wealth or who performed great services to the monarchy—usually both. One route to achieve noble status surpassed all others: royal office.

MIXING PRIVATE RIGHT WITH PUBLIC OFFICE on a salable and hereditary basis is so repugnant to the modern mind that historians find it difficult to regard the French *vénalité des offices*—as legal and generally accepted as it was during

(1970): 463–76, shows that one million *livres* invested in noncommercial form yielded enough revenue of a "noble" kind to allow one to live in the style of a noble. Chaussinand-Nogaret confuses things somewhat by referring to *roturier* fortunes as *mobilière*—true perhaps of the few test cases of great financial officers whom he is examining but certainly not of most *roturier* fortunes.

<sup>35</sup> The felicitous term of Durupt, *Propres*, 116, as a rationale for the system of *propres*.

<sup>36</sup> W. W. Rostow, *The Stages of Economic Growth*, 2d ed. (Cambridge, Mass., 1949): 4–5, ascribes this mentality to traditional societies (e.g., pre-Newtonian in the West). Taylor, "Noncapitalist Wealth," 482, applies it inaptly to Rostow's next period, when the preconditions for take-off are developed.

the last two hundred years of the Old Régime—as anything better than legitimized corruption.<sup>37</sup> In parts of the system private profit was always as great a motive as public service, to be sure. This was especially true of financial offices, where fortunes might be made quickly by those who collected royal taxes or disbursed royal monies. Involved was not embezzlement so much as exploitation by these officers of the financial power they derived from holding huge sums of public money in their private control over many months' time.<sup>38</sup> There were also farcical offices such as wig superintendent, which became the butt of many jokes. The most important part of royal officialdom, however, was the magistracy. Most of the magisterial offices, from the provincial court clerkships to the *Presidents à mortier* of the Parlement of Paris, were purchasable and hereditary, but few magistrates were corrupt or incompetent. The magistracy was the backbone of the administration for two hundred years, as vital to the constitution as the monarchy itself during the centuries when France was the greatest power in the western world.

We still lack a definitive study of the French system of venal-hereditary officeholding—what can be called “dynastic officialdom”—but we can appreciate how it served the nation in terms of public law.<sup>39</sup>

The English word “venal” carries a burden of opprobrium which for the most part should not be borne by French venal officeholding. A more proper connotation would be “legally available by purchase.” One of the most widespread misconceptions regarding hereditary officialdom is the belief that the potential evils of venality were reinforced when combined with those of hereditability. The truth is more that they mitigated each other. In the sixteenth century, when offices were still only venal, the officeholder was always in peril of losing his investment totally by inopportune death, for an office reverted to the crown if the holder did not sell it during his lifetime or resign it in favor of a relative at least forty days before his own demise. After 1604, when the *paulette* made offices hereditary, the individual's somewhat precarious title was transformed into an assured family possession. Therewith the reason for quickly drawing the maximum profit from offices was nullified by their classification as *propres* in the family heritage.

It can be argued also that venality nullified one of the dangers of heritability. For, if devolution were possible only to a son or other close heir, the odds were great that some generation would see the office forced upon an incompetent descendant—a plight encountered only too often in the history of dynastic monarchies. Venality allowed complete separation of the office from the family without loss of the investment if there were no suitable male heir;

<sup>37</sup> K. W. Swart, *Sale of Offices in the Seventeenth Century* (The Hague, 1949), 1–2, lists several who have the conventional attitude. Swart himself adopts it sometimes, although he is aware that the French system of offices was completely legal and was administered less corruptly than elsewhere; cf. *ibid.*, 34–35, 112, 121.

<sup>38</sup> See J. F. Bosher, *French Finances, 1770–1795* (Cambridge, 1970), 92–110.

<sup>39</sup> Paul Louis-Lucas, *Étude sur la vénalité des charges et fonctions publiques* (Paris, 1883), is fundamental for the juristic aspect of offices; see 5–73 for the history up to the *paulette*. Martin Göhring, *Die Ämterkäufllichkeit im Ancien Régime* (Berlin, 1938), covers the whole subject chronologically but is outdated. Roland Mousnier, *La vénalité des offices sous Henri IV et Louis XIII*, 2d ed. (Paris, 1971), is the most important historical work ever done on the subject, but its chronological scope is limited.

or, as one often sees in the history of great robe families, the office was transferred to a collateral line when the direct male descendant was an infant, was incompetent, or was unwilling. In brief, heritability attenuated venality's inclination to avarice by replacing it with family honor, while venality modified heritability's potential for incompetence by allowing a kind of *capacitat* to replace the usual *majorat*.

Venal-hereditary officeholding is subject to many more misconceptions in terms of its function in public law, but only one more can be mentioned here: the idea that a truly proprietary right was transferred from the king to the officeholder. As Loyseau declared in 1610, when heritability was just coming into use, it imparted "no right *in* the office, but a simple right *to* the office."<sup>40</sup> The king was always the proprietor and could repurchase the office at any time. The officeholder was the usufructur, and only that use was salable and inheritable. (Although women could not exercise an office, they could own the right to sell or assign one to a qualified man.) Two offices could not be held simultaneously, and the exercise of an office could not be sub-delegated to another person.<sup>41</sup> Free disposition by sale or testament imparted to offices the essential qualities of private property, which helps explain their classification as *propres*; but officers did not own public power, however much some of them late in the *ancien régime* acted as if they did. Dynastic officialdom never enjoyed the kind of seigniorial right in the exercise of public power that was characteristic of the feudal age.

As finally perfected the system of venal-hereditary office worked analogously to *rentes*: as long as the crown never repurchased offices—and, if it ever had the will, it lacked the money to do so on a broad scale—they were *de facto* permanent family possessions, properties that gave splendor to the family's name because they involved the exercise of public power.<sup>42</sup>

It is not true that offices were a great source of revenue, save for financial officials. A comprehensive study of the market value of various offices still needs to be done, but the abundant examples we have show consistently that offices were the lowest-yielding element of the family fortune. The greatest

<sup>40</sup> *Offices*, III, ii, § 2 (ed. *Oeuvres* [Paris, 1666], 233).

<sup>41</sup> The kind of *cumul des offices* that Pierre Goubert talks about ("Les officiers royaux des présidiaux, bailliages et élections dans la société française du XVII<sup>e</sup> siècle," *XVII<sup>e</sup> siècle*, 42–43 [1959]: 54–65) involved "moonlighting" by royal or municipal officers as magistrates in rural seigniorial jurisdictions. I know of no instance of two royal venal-hereditary offices being held simultaneously by one person.

<sup>42</sup> The Parlements of Normandy (in 1756) and Bordeaux (in 1771) claimed proprietary rights in offices "autant que cette des rentes et des héritages," but such touting of public office as lineage property does not prove that the law had changed; see Jacques Kubler, *L'origine de la perpétuité des offices royaux* (Nancy, 1958), 279, n. 21, and 263–94. The long practice of hereditary officialdom created a mentality of possessiveness—enhanced, of course, by such things as Montesquieu's defense of venal office (*L'esprit des lois*, 5: 19)—but the words and acts of Maupeou in 1771 show that the crown did not accept the perpetuity of Parlement and its officials. Goubert, *Ancien Régime*, 2: 143, opines that the sale of offices could be considered as loans. Loyseau, *offices* [1610], III, I, §§93–95 (ed. *Oeuvres* [Paris, 1666], 231), points out that this was literally true in the sixteenth century; but the *paulette* changed things radically. For *ancien régime* testimonials on the role of offices in patrimonies, see B. N. ms. fr. 7012, fol. 495v (anonymous mid-eighteenth-century pamphlet on venality); Jacques Leschassier, *Discours de rendre les offices héréditaires et matrimoniaux tenus en fief du roy* [ca. 1610], ed. *Oeuvres* (Paris, 1649), 235–39; Denis Lebrun, *Traité des successions* [1692], II, iii, 7, § 25 (ed. Paris, 1700: 253)—a work of fundamental importance; J.-M. Ricard, *Traité des donations entre vifs et testamentaires* [1632], 3<sup>e</sup> partie, X, 1, § 1424 (ed. Paris, 1707, 1: 740).



offices, such as the *Presidents à mortier* of the Parlement of Paris, could even drain family wealth because revenue from them was insufficient to maintain the style of life expected of the official. Office could not be a predominant element in the family fortune: it averaged no more than twenty percent of it, often much less (financial offices excepted as usual).<sup>43</sup> From the economic point of view, therefore, offices were an even more conservative element in *propres* than land or *rentes*, not to mention their equally great immobilization of family capital.

The insatiable thirst for offices was dubbed *archomanie* in a memorable chapter of Loyseau's *Offices* in 1610, and was satirized by Ponchartrain a century later when he quipped to Louis XIV that every time the king created an office God created a fool to buy it.<sup>44</sup> This phenomenon can be explained reasonably only in terms of the status that office provided, and above all the possibility of noble status itself.

Status through office was achieved in two different stages: 1.) the rise while still *roturier* from a lower- to a higher-ranking office that was not noble; and 2.) the change from *roturier* to noble status by holding for three generations an office that was ennobling. The initial process of climbing the lower rungs on the ladder of offices necessarily involved the venal element. All offices were *propres* in the law, but alienating a lesser one for another with higher status was normal family practice. The long-range plan was to improve the family's "official" status until it acquired an office whose holder was personally regarded as noble during his tenure. Then the factor of heritability became paramount: after three generations the officeholder's personal noble status became the birthright of his entire family. Legally all the offspring of the third generation were noble whether or not they held office.<sup>45</sup> This juridical three-generation rule for upward mobility in the social order of the *ancien régime* has its counterpart in the sociological three-generation model of mobility from lower- to upper-class status in modern society.<sup>46</sup> Wealth is the necessary basis, but acculturation requires manners and bearing which only the third generation of the rising family acquires from infancy. In the seventeenth century the three-generation rule was defended by a pseudogenetic theory that claimed it

<sup>43</sup> Philip Dawson, *Provincial Magistrates and Revolutionary Politics in France, 1789-1795* (Cambridge, Mass., 1972), 89-91, estimates that *bailliage* and *présidial* offices comprised sixteen percent of the value of family fortunes but yielded only half that percentage in terms of total annual income. Goubert, *Beauvais*, 328, estimates that offices constituted one-fourth of family fortunes, but in another work (above, note 41, 66-68) notes that the proportionate size of office in family fortunes decreased during the seventeenth century (except for lucrative financial ones) and that office yielded poorly compared with other investments. See also Mousnier, *Vénalité* (2d ed.), 74-77, in general. For the profitability of financial offices, see Chaussinand-Nogaret (above, note 34) and Boshier, *French Finances*, 67-91.

<sup>44</sup> Loyseau, *Offices*, III, i, § 10 (ed. *Oeuvres* [1666], 223). Marcel Marion, *Dictionnaire des institutions de la France aux XVII<sup>e</sup> et XVIII<sup>e</sup> siècles* (Paris, 1923), 406.

<sup>45</sup> The three-generation principle (derived originally from Roman Law, *Cod.* 12, 1, 1) was used as a prescriptive method to claim nobility after three generations of "living nobly" until replaced in the sixteenth century by officeholding as the surer route to nobility; see J. R. Bloch, *L'anoblissement en France au temps de François I<sup>er</sup>* (Paris, 1934), 30 ff., who also provides a list of ennobling offices (76 ff.). The state of ennobling offices in the later eighteenth century is admirably set forth by David Bien, "La réaction aristocratique avant 1789," *Annales (E.S.C.)*, 29 (1974): 23-48, 505-34. Other more or less anecdotal reports of the ennobling process are provided by Goubert (as above, n. 41, 73-75); Mousnier *Vénalité*, 2d ed., 77-83; Dawson (*Provincial Magistrates*, 58-60); Elinor G. Barber (*The Bourgeoisie in 18th-Century France* [Princeton, 1955], 99-140); and Vivian R. Gruder (*The Royal Provincial Intendants* [Ithaca, 1968], 52-70).

<sup>46</sup> Pitirim Sorokin, *Social and Cultural Mobility*, XVII, §§ 8-14 (ed. London, 1964: 449-57).



took that long to breed out ignoble elements,<sup>47</sup> but the obvious environmental explanation, which needed no touting, was that only children of the third generation, born and raised in a milieu where the family head was noble *ex officio*, would live their whole lives in noble circumstances.<sup>48</sup>

The dependence of office upon other forms of wealth is shown by the fact that a fortune five to six times greater than the value of the office itself was necessary to support the style of life it required.<sup>49</sup> Public service without appropriately large private means was simply unthinkable, and service in office was basically incompatible with commercial enterprise—indeed, such was forbidden to those holding offices with noble attribution. Income from land and *rentes*, therefore, was the main support of the officeholder's family, and these were "noble" forms of wealth. Upward mobility over several generations by means of office complemented the multigenerational increase of the family fortune by conservative augmentation of *rentes*. The latter worked minimally to assure survival of future generations, while office worked maximally to gain nobility by birth for descendants.

The model of compounding *rentes* required self-sacrifice by each generation, a mentality that still needs to be proved by more than anecdotal study of the growth of family fortunes; but the three-generation process of attaining family tenure of nobility demonstrates *prima facie* the cooperation of many generations to give social status to descendants whom they would never live to see. In an age when life-chances were so completely determined by family inheritance, this conjunction of the economic and political—i.e., "official"—destinies of the family heritage encouraged discipline by each generation for the benefit of future generations. *Rentes* may seem to be sublimated usury and venal-hereditary office merely camouflaged corruption, but they were, together with land, the most respectable forms of wealth and the principal elements in the rise of great families. The laws of succession which bound *rentes* and office together as *propres* in inheritance add a legal dimension to our understanding of this phenomenon.

SINCE POLITICAL THINKERS of the *ancien régime* regarded strong families as pillars of the state and jurists regarded the system of *propres* as a unique way to

<sup>47</sup> Jer. Ximenez de Urrea, *Dialogues du vray honneur militaire* (Paris, 1585), 172v, cited in André Devyver, *Le sang épuré: les préjugés de race chez les gentilshommes français de l'Ancien Régime* (Bruxelles, 1973), 167.

<sup>48</sup> This reasoning was pre-empted by the acquisition of nobility for all his family by a new *secrétaire du roi* who died in office or possessed it for twenty years (increasingly common in the eighteenth century—see Bien, "Réaction aristocratique," 43 ff.), but the original cost of those offices and the demands for loans put by the king upon those who held them meant that only the very rich *roturier* could qualify; see Chaussinand-Nogaret, as above, n. 34, and the forthcoming article by David Bien, "The *Secrétaires du Roi*: Absolutism, Corporatism and Privilege under the Ancien Régime."

<sup>49</sup> See above, note 43. If the bulk of the income from other lineage property were required to support the officeholder in a respectable style of life (as would often be the case with families of modest wealth), then the family with many children would be forced either to advantage the one child succeeding to the office (which was indivisible) or to sell the office and divide the proceeds among all of them. Between these alternatives, nearly everyone would choose the former, I do believe, just as a peasant holding only one workable plot and having many children would somehow evade all laws of equal division and manage to have that plot pass undivided to one child; see Lutz K. Berkner and Franklin F. Mendels, "Inheritance Systems, Family Structure, and Demographic Patterns in Western Europe (1700–1900)," forthcoming in *Historical Studies in Changing Fertility* (Princeton University Press), ed. Charles Tilly.

preserve the strength and glory of the family,<sup>50</sup> it may be assumed that the *coutumiers'* system of lineage property was generally accepted as a stabilizing force in the French nation. Yet no scholar appears to have evaluated the system of *propres* in terms of legal philosophy, let alone national stability.

Seen looking forward from the Middle Ages, lineage property appears to be an evolution from extended-family proprietorship, devoted to preserving the transmitted heritage, toward a system based upon the stability and growth of new nuclear families. Seen looking backward from the Civil Code of 1804, with its emphasis upon individual property rights, the system of *propres* is clearly premodern because it restricted the freedom of successive heads of the family in favor of the perpetual corporate integrity of the lineage. The sense of private property is fully developed, one can say, but it is not yet individualistic. The underlying mentality is not "possessive individualism," as Macpherson has characterized seventeenth-century English thought, but rather "family possessiveness."<sup>51</sup>

Temporal continuity, the essential quality of such "family possessiveness," was well served when perpetual annuities and perpetual offices were embraced by the system of *propres*. Economic, political, and social institutions were thus yoked together under the law, and the *roturier* families lucky enough to rise above the marginal life that was the lot of most were equipped to aggrandize themselves over the generations. If too multifarious to be called an "order" of society in the old corporatist sense and too inchoate to have the self-consciousness of a "class" in the socialist paradigm, this group of families must be regarded in the first place, from any point of view, as essentially a stabilizing element in the civil order of the nation.

The stability which *rentes* and offices provided in the family heritage (and vice versa) is self-evident, but some of the ways that they worked to provide stability for the nation as a whole are perhaps not obvious. First of all, *rentes* promoted various kinds of interfamilial relationships. *Rentes foncières* were extraordinary in that they created two *propres*: in the debi-renter's heritage because he was the true new proprietor of the land or house upon which the *rente* was established, and in the credi-renter's heritage because the security for the *rente* was a specific immovable.<sup>52</sup>

*Rentes constituées* established a *propre* only in the heritage of the credi-renter, for the capital sum the debi-renter received was a *meuble*; but there was

<sup>50</sup> For political thought see Jean Bodin, *Les six livres de la république*, I, 2, and the antiquarian Etienne Pasquier, *Recherches de la France*, II, 18; for juristic thought see André Tiraqueau, *De iure primogeniorum* Q, 23, § 12; Claude de Ferrière, *Corps et compilation de . . . la Coutume de Paris*, gl. 1 on tit. xv, art. 312, and François Bourjon, *Le droit commun de la France et la Coutume de Paris réduits en principes*, XV, 3, § 1.

<sup>51</sup> C. B. Macpherson, *The Political Theory of Possessive Individualism, Hobbes to Locke* (Oxford, 1962). Compare this observation by René Filhol, "Propriété absolue et démembrement de la propriété dans l'ancien droit français," *Travaux et recherches de l'Institut de droit comparé de l'université de Paris*, 22 (1963): 43: "Revolutionary legislation attached property to the individual, but individualism appeared quite tardily in our history; . . . old French law knew it only very late and incompletely, always refusing to detach the individual juridically from the social group or corps to which he belonged."

<sup>52</sup> "Dédoublement de la propriété foncière" is Charles Lefebvre's felicitous expression in *Rentes perpétuelles*, 44—the only author who in my recollection stresses this fact which is so important for anyone trying to account for proprietorship. How often have estimates of landholding in prerevolutionary France counted the same piece of property twice, if there were a *rente foncière* established on it?

nothing invidious in the relationship. It was quite common to be a creditor in one relationship and debitor in another; moreover, these *rentes* enjoyed a much greater number and extension than any other kind. "Everyone who was not poor, from well-off laborers and small shopkeepers up to princes of the blood and even the king himself, were involved in giving and taking—often both at once—in the extraordinary network of *rentes constituées* which stretched out over all the kingdom."<sup>53</sup> Such interlocking family heritages may have been forced upon France chiefly by the laws against usury, but the unexpected results as a socially integrative force over the centuries begs for the historian's advertency.

The function of offices in binding family heritages to public power is just as provocative. One eighteenth-century defender of venal-hereditary officeholding went so far as to maintain that the aspiration to hold office was a mainspring of the economy: the *négociant* acquired wealth in the hope of raising his family to the magistracy, which in turn encouraged the *marchand* to do the same, and so the dream of office gave high purpose to commerce.<sup>54</sup> If this chain of reasoning about socioeconomic behavior seems odd, the writer at least testifies to what every modern historian seems to believe, that the French were inclined to get out of business when the chance for high official status or nobility was offered. That they got into business in order to get out of it quickly when office or nobility became available is not easy to prove, but not impossible to believe.

Less easy to dismiss is the statistic that two to three percent of adult males possessed some office on the eve of the revolution.<sup>55</sup> This blending of family possession and public function presumably committed tens of thousands of families to upholding the status quo. Yet we must be prudent. Was the mentality of the new generations of officeholders, those not yet on the track leading to nobility or perhaps nearly or newly ennobled, the same as the mentality of families that had long since achieved noble status? The eighteenth-century *parlementaire* attitude of "*L'état, c'est nous*" shows that venerable nobles of the robe had transcended their official obedience to the king. The shift of allegiance from king to nation so often noted is to be found as much among *officiers* as among *philosophes*.<sup>56</sup>

<sup>53</sup> Goubert, *Beauvais*, 548. Goubert provides an example of a *rente constituée* with its blanket-mortgage characteristic in *L'Ancien Régime*, 1: Ch. 6, *ad finem*.

<sup>54</sup> B. N. ms. fr. 7012 (see above, n. 42), fol. 493r. The jurist Denisart at the same time put things more practically, noting how the state would suffer loss of revenue (from the *pauvette*) if it did not maintain public confidence in offices; *Collection de décisions nouvelles* (s.v. "*Amovibilité*") cited by Kubler (above, note 42), 273, n. 26.

<sup>55</sup> Taylor, "Noncapitalist Wealth," 477. Necker estimated 51,000 offices in 1778, but this was limited to the courts, the municipalities and the financial system. He valued them (if repurchased) at 600,000,000 *livres*, based upon the 1771 declaration of the officeholders themselves; but we know that they undervalued their offices in this survey (used to set a tax basis) by as much as fifty percent, so that 1,000,000,000 *livres* is a conservative estimate of market value on the eve of the revolution. About one-third of this sum was appropriated in 1790 for the repurchase of offices, and some 235,000,000 *livres* in assignats were finally issued those who surrendered their offices; see Dawson, *Provincial Magistrates*, 255–74. Louis-Lucas, *Vénalité*, 526–51, follows through all the revolutionary laws liquidating offices.

<sup>56</sup> François Olivier-Martin, *L'organisation corporative de la France d'ancien régime* (Paris, 1938), 401–42, assesses the spirit of corporate unity and cooperation among royal officers, providing some good material

Dynastic officeholding committed a family to rearing its children with the expectation of leadership and to providing them with the education needed to realize it. If this tended to become a closed system, repugnant to our notions today of a civil service open to merit, it was, nevertheless, a pragmatic way to provide a body of officials suitable for public service in an era of limited higher education. There is no reason why the formulation "family honor and public service" that is applied so facily to feudal seigneurie should not be used also to describe the dynastic officialdom that replaced it.

The national character of dynastic officialdom shows itself also in the great lateral and upward career mobility of most officials—with Paris always as the central magnet—not only in their own lifetimes but even more so in the histories of their families. This mobility was based chiefly upon their common competence in legal training, which also accounts for the final triumph over the diversity of laws that had frustrated national unification from time immemorial. The search for a "common law of customary laws,"<sup>67</sup> combined with the rapprochement between those *coutumiers* and Roman law (the laws of succession provide a good example), exemplify on the part of officialdom a striving for national integration that deserves to be put alongside the vaunted state-building of the Bourbon kings.

THE SCHEMA OF RELATED HYPOTHESES set forth here has a demonstrably sound basis in law, and anecdotal proofs could readily be provided to show its operation. A true test of its merits, however, will not be possible until notarial and juridical archives are systematically exploited by researchers fully versed in the inheritance laws of the *ancien régime*.<sup>68</sup> Only then will the function of *rentes constituées* in tying together family heritages become clear, the cynosure of office in family heritages be proven, and the dynamic role of the system of *propres* in molding family fortunes be verified.

The laws of inheritance, Tocqueville said (continuing the quotation with which we began), "have a sure and uniform manner of operating upon society; in a fashion, they seize generations before they are born. Man is armed by them with an almost godlike power over the future lot of his fellow men." In France the law had to be manipulated to protect family heritages from the partition which the letter of the law required, but the system finally allowed a conjuncture of economic power in the form of land and *rentes* with

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for understanding the mentality of dynastic officialdom; but he turns against these officers when they stepped out of their corporate confines and began to assert public responsibility, as the *parlementaires* did in the eighteenth century. These pages are imbued with the scent of modern corporatist ideology.

<sup>67</sup> The work of Bourjon mentioned above in note 50 is the most comprehensive effort along this line. For the whole story, see André-Jean Arnaud, *Les origines doctrinales du Code Civil Français* (Paris, 1969).

<sup>68</sup> Pierre Goubert has regularly voiced the hope for a great team effort to do this, notably in his *Beauvais*, 538, in connection with the network of *rentes constituées* (see above, note 53). Jean Yver begins and ends his work on *Egalité entre héritiers* with plaintive remarks about the lack of knowledge of *pratique* (5–7, 296–97). If I understand Jean Meyer correctly (*La noblesse bretonne au XVIII<sup>e</sup> siècle* [Paris, 1966], 1: 128), most documents regarding successions were never publicly registered, or were done so only in bits so as to preserve family secrets.

political power in the form of office, producing a largely new order of ruling families. The French Revolution swept away the heritability of political office, as the American Revolution had done earlier. Private property nevertheless remained sacrosanct in both countries, and (*pace* Tocqueville) other ways were found to keep family fortunes intact through many generations.

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## The Organized Peace in Southern France and Catalonia, ca. 1140 – ca. 1233

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THOMAS N. BISSON

IN THE HISTORY OF POLITICAL POWER in southern France and Pyrenean Spain, the turn of the thirteenth century seems to mark a forbidding divide. For more than a hundred years castle-based elites had flourished in dynastic or military alliances of bewildering mutability, weakening the ancient solidarities of Carolingian territorial order. Principalities had arisen or persisted but these were dynastic combinations lacking administrative institutions capable of welding disjointed lands into effective political units. Much of this was now to change. The old houses of Toulouse and Béziers-Carcassonne together with the trans-Pyrenean designs of the Aragonese-Catalan dynasty collapsed in the Albigensian crusades. As early as 1212 Simon de Montfort organized a quasi-colonial government for lower Languedoc based largely on the baronial custom of “France near Paris”; the county of Toulouse was to succumb more gradually but no less inevitably to the rule of ambitious Frenchmen in the next generations; while the fiscal insolvency of Peter of Aragon, followed by his untimely death on the battlefield of Muret (1213), obliged his successor James the Conqueror to found his new imperialism against less dangerous Mediterranean neighbors upon an improved politics and finance. Historians have understandably stressed the hard new realities of this spectacularly changed world. Whatever their sense of the cultural losses, they have made no claims for a native political order proven incompetent by the test of war. The persistent disorders of the later twelfth century, in Catalonia as well as in Languedoc, only seemed to confirm other evidence of a considerable political failure on the part of contemporary Mediterranean regimes.<sup>1</sup>

Based on a paper presented to a symposium in honor of Joseph R. Strayer at Princeton University on March 31, 1973. Abbreviations: *ACP* = *Recueil des actes des comtes de Provence appartenant à la maison de Barcelone*. Alphonse II et Raimond Béranger V, ed. Fernand Benoit, 2 vols. (Monaco, 1925), 2; *HL* = Claude Devic and J.-J. Vaissete, *Histoire générale de Languedoc* . . . , new ed., 16 vols. (Toulouse, 1872–1904); *LTC* = *Layettes du Trésor des Chartes*, ed. Alexandre Teulet et al. 5 vols. (Paris, 1863–1909); *PL* = *Patrologiae cursus completus* . . . *latinae*, ed. J.-P. Migne, 221 vols. (Paris, 1844–64). The terms “Occitania” and “Occitan” are sometimes used in reference to lands of southern France extending beyond yet not exclusive of the historical “Languedoc.”

<sup>1</sup>For prevailing views, see Edgard Boutaric, *Saint Louis et Alphonse de Poitiers* . . . (Paris, 1870), 13–40; Auguste Molinier, “Étude sur l’administration féodale dans le Languedoc (900–1250),” in *HL*, 7: esp. 142–45, 147 n. 1, 150–52, 155 ff., 191–213; Molinier, “Étude sur l’administration de Louis IX & d’Alphonse de Poitiers (1226–1271),” in *HL*, 7: 462–63, 466–67; Yves Dossat, “Le comté de Toulouse et la féodalité



The trouble with this perspective, for all its merits, is that it has distorted our understanding of meridional government in the age before the Occitan crusades. It has disposed us to imagine a feudalism in Mediterranean lands that was somehow inferior to that of the tough northern barons and knights. But external danger was one thing, internal security quite another. Contemporaries in southern France and Catalonia had no idea of the political disasters which lay ahead, and if their leaders do not seem to have insisted on vassalic fidelities or feudal obligations to combat disorder, it may have been because they had other means. Although no thorough investigation of these societies has yet been made, it is becoming increasingly clear that feudalism developed too late in the South entirely to replace the older structures of territorial power, and that its political significance can easily be exaggerated. Even in Catalonia, where a thoroughgoing custom of vassalic fidelity and fiefs had evolved by 1100, there is little indication that the counts of Barcelona sought to systematize this custom to the advantage of central authority, as did some "feudal monarchs" in northern Europe. On the contrary, the *Usatges of Barcelona* are primarily a regalian code in which the definition of obligations for the maintenance of public order has little to do with the law of fiefs.<sup>2</sup> Harder to exaggerate, accordingly, is the political significance of the peace movements. In Catalonia the counts and prelates had for generations relied upon the statutory order of the Peace and Truce of God to supplement the guarantees of vassalic conventions, and these efforts, plainly visible in the *Usatges*, were renewed in the reigns of Alphonse II (1162–96) and Peter II (1196–1213). The former instituted vicars as secular judges of the peace in collaboration with the bishops, and peasant militias to enforce the peace in the dioceses; moreover, he and his son collected compensations for the maintenance of the rural peace and of the stability of coinage. And since criminal justice in Catalonia remained poorly distinct from the jurisdiction of the peace, it can be said with little exaggeration that the constitutional order of the realm was basically the statutory structure of the peace.<sup>3</sup>

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languedocienne à la veille de la croisade albigeoise," *Revue du Tarn*, new ser., 9 (1943): 75–90; Pierre Timbal, *Un conflit d'annexion au moyen âge: l'application de la coutume de Paris au pays d'Albigeois* (Toulouse-Paris, 1950); Philippe Wolff, *Histoire du Languedoc* (Toulouse, 1967), chs. 5, 6; Ferran Soldevila, *Història de Catalunya*, 2d ed. (Barcelona, 1962–63), chs. 9–12, and esp. page 243; and J. R. Strayer, *The Albigensian Crusades* (New York, 1971), ch. 1 (esp. pages 4–5, 11). However, A. R. Lewis, "The Formation of Territorial States in Southern France and Catalonia, 1050–1270 A.D.," *Mélanges Roger Aubenas* (Montpellier: *Recueil de Mémoires et Travaux publiés par la Société d'Histoire du Droit . . . des Anciens Pays de Droit Écrit*, 9, 1974), 507–09, stresses institutional continuity and even "a more centralized, feudalized control" at the turn of the twelfth century. The recent work most helpfully related to the subject of this article is *Paix de Dieu et guerre sainte en Languedoc au XIII<sup>e</sup> siècle* (Toulouse: *Cahiers de Fanjeux*, 4, 1969), wherein the complications of religious and political problems in the generation before the crusade are well stressed. For other pertinent work on the Peace, see note 4.

<sup>2</sup> See generally Élisabeth Magnou-Nortier, *La société laïque et l'église dans la province ecclésiastique de Narbonne . . . de la fin du VIII<sup>e</sup> à la fin du XI<sup>e</sup> siècle* (Toulouse, 1974); and cf. Pierre Bonnassie, *La Catalogne du milieu du X<sup>e</sup> à la fin du XI<sup>e</sup> siècle* . . . , 2 vols. (Toulouse, 1975–76), esp. 2, chs. 9, 11–13. For Catalonia, my observation is based on the redating of the *Usatges* to the mid-twelfth century by Ramon d'Abadal and Bonnassie (see *Catalogne*, 2, 711–28) and on a comparative study of feudal monarchies presented before the Mediaeval Academy of America in April 1975 and now being prepared for publication.

<sup>3</sup> *Cortes de los antiguos reinos de Aragón y de Valencia y principado de Cataluña*, 26 vols. (Madrid, 1896–1922), 1:1, 55–88; Eugen Wohlhaupter, *Studien zur Rechtsgeschichte der Gottes- und Landfrieden in Spanien* (Heidelberg, 1933), sections 8–10; Sister Karen Kennelly, "Catalan Peace and Truce Assemblies," *Studies in Medieval Culture*, 5

Now an order of this general description was not simply a renewal of the ancient institutes of the Peace and Truce nor was it unique to Catalonia. It was imposed in southern France and, indeed, in some parts of the North and of Spain, only in the second half of the twelfth century, with a few (but remarkably few) anticipations before that; it represented a major associative effort to remedy the weakness of lay powers in dealing with violence in much of the South; and its influence was to survive the Albigensian crusades. It marked a significantly new stage in the evolution of the medieval Peace: the stage in which the maintenance of security is made to rest on the instituted obligations in service and money of the regional community.<sup>4</sup> To make this point clear, it will be necessary to refer to what appear to be the earlier perceptible structures of the Peace of God.

THE DISORDERED SOCIETIES OF THE TENTH CENTURY had inherited—and increasingly ignored—the prescriptions for territorial security recorded in Germanic codes and capitularies.<sup>5</sup> Only in England were these prescriptions maintained in the tenth and eleventh centuries. In West Frankland, despairing clergymen assumed not only the burden of restoring the Carolingian institutes of peace and justice no longer enforced by the kings, but also that of pacifying the brutalized populations whose leaders were wrecking the county. And the instrument of pacification was found in the cult of relics. Writing of the lay saint Gerald, who had brought a relic of the True Cross to Aurillac, Saint Odo of Cluny observed, “The inhabitants of that region had truly ferocious habits, but gradually by his example and the reverence they have for the holy man they seem to be gentler. When they make any agreement or solemn oath in law, they have the relic brought by some monk or cleric. . . .”<sup>6</sup> In such ways the strongly penitential faith of the monks was propagated even in remote localities and peasants were associated with magnates in the moving experience of mysterious power. In the Limousin, the “pact of peace and justice” was to be linked with the devotion to Saint-Martial, and there is

(1975): 41–51; Jesús Lalinde Abadía, *La jurisdicción real inferior en Cataluña . . .* (Barcelona, 1966), 70–72; T. N. Bisson, “An ‘Unknown Charter’ for Catalonia (A.D. 1205).” *Album Elemér Mátyusz* (Brussels: *Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions*, number 56, 1976), 61–76; and see below, page 310.

<sup>4</sup> A similar thesis was advanced many years ago by Georges Molinié, *L’Organisation judiciaire, militaire et financière des associations de la paix. Étude sur la Paix et la Trêve de Dieu dans le Midi et le Centre de la France* (Toulouse, 1912), esp. 32–40. But Molinié’s evidence was very incomplete; moreover, it was badly pressed to argue the existence of fully organized associations of the peace. Roger Bonnaud-Delamare had little difficulty refuting Molinié on this point, “Le légende des associations de la paix en Rouergue et en Languedoc au début du XIII<sup>e</sup> siècle (1170–1229),” *Bulletin philologique et historique . . . du Comité des Travaux historiques et scientifiques* (1936–37), 47–78; but neither he nor subsequent historians seem to have recognized the merit of Molinié’s underlying perception, which was discredited with the rest. Cf., e.g., Sister [Karen] Kennelly, “Medieval Towns and the Peace of God,” *Medievalia et Humanistica*, no. 15 (1963): 35–53 (esp. 40, 42).

<sup>5</sup> E.g., *Capitularia regum Francorum*, ed. Alfred Boretius and Victor Krause, 2 vols. (Hanover: *Monumenta Germaniae Historica, Legum Sectio II*, 1883–97), 1: nos. 68, 69, 77; 2: no. 267. On the origins of the Peace of God, see Hartmut Hoffmann, *Gottesfriede und Treuga Dei* (Stuttgart, 1964), chs. 1–4.

<sup>6</sup> *PL*, 133: 700–01; I follow the translation by Gerard Sitwell, *St. Odo of Cluny . . .* (London, 1958), 177.

record of such incidents scattered from the upland churches of south central France to Flanders and Normandy.<sup>7</sup>

What was thus manifested, from the present point of view, was a characteristic associative structure that may be called the "sanctified peace."<sup>8</sup> Monastic in inspiration, it was based on the shared religious emotion of the united estates of people, vented in waves of popular enthusiasm most apparent in the 990s, 1020s, and 1030s. It was not altogether devoid of institutional consequence, for it was on occasions such as have been mentioned that oaths to the peace were first sworn, although it is by no means clear that the oath was routinely administered to all who attended reliquary ceremonies. But this structure, or experience, was inherently unstable. The sworn men were hard to control, especially when mobilized: the excesses of the archbishop's throng in Berry (ca. 1038), to take a famous instance, can only have confirmed the natural suspicions of magnates and knights.<sup>9</sup> Incidents rather than institutes, recorded exclusively in monastic or episcopal narratives and sermons, the sanctified peace waned after 1050 or so, although it had a fitful recurrence here and there, notably in the uplands of southern France, in the twelfth century.

Very different in character and influence was the instituted peace. To restore peace, as the leaders of the movement had soon recognized, was, in some fashion, to make peace, to legislate, so that the ceremonial affirmations of the peace soon came to be accompanied by written definitions according to episcopal models. "This peace," say the statutes, or "this truce and peace,"<sup>10</sup> and progressively the territorial incidence of the statutory peace was laid down. Now for the making of such regulations, which are silent about ceremony, there could be no place for the common folk, who were before long also being exempted from the oaths of peace. The negative, injunctive language of such oaths was irrelevant to those who were to be protected, and the definitions of limited or just war that can be extrapolated from the written oaths that have come down to us read like a clerical lesson to the nascent order of knights in the eleventh century.<sup>11</sup> But not even the knights were to have much part in founding the peace. The lay men associated with the bishops and abbots were usually princes (*principes*) or magnates, those who alone besides the king had the regalian powers that the prelates were committed by their formation to uphold.

<sup>7</sup> Adémar de Chabannes, *Chronique*, book 3, ch. 35; ed. Jules Chavanon (Paris, 1897), 158; and generally Hoffmann, *Gottesfriede*, 18–20, 25–31, 40, 51–53, 134–35, 143–44, 167.

<sup>8</sup> The phenomenon has been much discussed: see, e.g., Carl Erdmann, *Die Entstehung des Kreuzzugsgedankens* (Stuttgart, 1935), 66–67; Bernhard Töpfer, *Volk und Kirche zur Zeit der beginnenden Gottesfriedensbewegung in Frankreich* (Berlin, 1957); H. E. J. Cowdrey, "The Peace and the Truce of God in the Eleventh Century," *Past and Present*, no. 46 (1970): 44–53; as well as Hoffmann, as cited in note 7.

<sup>9</sup> Hoffmann, *Gottesfriede*, 105–07; the incident is reinterpreted by Guy Devailly, *Le Berry du X<sup>e</sup> siècle au milieu du XIII<sup>e</sup>* . . . (Paris-La Haye, 1973), 145–48.

<sup>10</sup> E.g., J. D. Mansi, *Sacrorum conciliorum nova et amplissima collectio*, 31 vols. (Florence-Venice, 1759–98), 19: 593–96; Hoffmann, 260–62.

<sup>11</sup> E.g., C. J. Hefele, *Histoire des conciles* . . . , tr. Henri Leclercq, 10 tomes in 20 vols. (Paris, 1907–38), 4:2, 1409–10; Christian Pfister, *Études sur le règne de Robert II le Pieux (996–1031)* (Paris, 1885), lx–lxi; and see generally Georges Duby, "Les laïcs et la paix de Dieu," reprinted in *Hommes et structures du moyen âge* (Paris-La Haye, 1973), 231–37.

It was no purpose of the early instituted peace to replace existing institutions of justice and police (such as they were). Neither courts nor taxes for the peace are mentioned in extant statutes of the Aquitanian councils, nor apparently in those which spread from diocese to diocese, perhaps as far as Ausona, with the encouragement of Robert the Pious in the 1020s and 1030s; not even the specification in these statutes of episcopal jurisdiction with spiritual sanctions over the “broken peace” was altogether new.<sup>12</sup> Only in the council held by Duke Guilhem V at Poitiers some time before 1015 do we find an army of enforcement being instituted, yet even there the thrust of the legislation was to oblige the “princes”—that is, the rulers of the counties—to do justice themselves, or through their judges, over violators of churches.<sup>13</sup> Thereafter, the only armies mentioned (and very rarely) in statutes or oaths are the regalian or vassalic forces that existed independently of the peace.<sup>14</sup> The hostages exchanged by magnates upon confirmation of the peace<sup>15</sup> evidently grew out of the customary practice in judicial (or quasi-judicial) concords by which private wars were ended. Since the peace in this form—structurally traditional—was no threat to regalian power, was if anything a form of reinforcement of it, we can easily understand why the princes of Flanders, Normandy, and Barcelona promptly followed the lead of Guilhem V. In their hands the written peace began to change in diplomatic form, but in substance and coverage it remained surprisingly faithful to the clerical models.<sup>16</sup>

The instituted peace seems to have lost impetus in the later eleventh century. While the synodal statutes remained theoretically in effect and were reconfirmed here and there, their principal defect now became clear: lacking explicit provision for temporal support, they could only be enforced where prelates and lay lords were disposed to act in harmonious determination. The obvious remedy for this defect lay in extra-statutory contrivances, of which some important examples are attested: the bishop’s militia in the North and East,<sup>17</sup> judges of the peace in Gévaudan,<sup>18</sup> taxation to support the peace in Périgord (and possibly Berry),<sup>19</sup> and the occasional administration of oaths to

<sup>12</sup> See Hoffmann, *Gottesfriede*, ch. 4, and 20–21, 260; cf. 22, 245–46.

<sup>13</sup> Only the failure of the secular jurisdiction would be cause for reconvening the magnates as an army, Mansi, 19: 267–68: “. . . princeps vel iudex, ipsius rei aut iustitiam faciat aut obsides perdat; et si iustitiam facere non potuerit, convocet principes et episcopos qui concilium instituerunt et omnes unanimiter in destructionem et confusionem ipsius pergant. . . .”

<sup>14</sup> Hefele-Leclercq, *Conciles*, 4:2, 1409; Hoffmann, *Gottesfriede*, 261. For a good example in the diocese of Urgel (ca. 1104), see Petrus de Marca, *Marca hispanica* . . . (Paris, 1688), appendix, no. 333: the count of Pallars promised the bishop “ut si quis treguam Dei vel pacem infregit aut infregerit, toto posse, tota voluntate per se & per suos adiutorium Episcopo tamdiu impendat donec per distractionem ad satisfactionem seu ad justam emendationem veniat.”

<sup>15</sup> E.g., at Poitiers, early eleventh century, Mansi, 19: 267–68.

<sup>16</sup> Hoffmann, *Gottesfriede*, chs. 9, 10; Wohlhaupter, *Gottes- und Landfrieden in Spanien*, 65–101. For some characteristic texts, see *LTC*: 1, no. 22; Hoffmann, 260–62; *Cortes de Cataluña*, 1:1, 49–51, 55–62.

<sup>17</sup> Hoffmann, *Gottesfriede*, 104–10, 176, 197; Hugues de Flavigny, *Chronicon*, ed. G. H. Pertz, *M.G.H., Scriptores*, 7 (Hanover, 1848): 477–78.

<sup>18</sup> Clovis Brunel, “Les juges de la paix en Gévaudan au milieu du XI<sup>e</sup> siècle,” *Bibliothèque de l’École des Chartes*, 109 (1951): 32–41.

<sup>19</sup> Hoffmann, 90–106; cf. below, page 300, at note 56. (The instituted redemption of the rural peace in Cerdanya is dealt with below; Hoffmann to the contrary notwithstanding, p. 119, nothing comparable is attested in Catalonia until late in the twelfth century.)

the barons in Burgundy, Berry, and Béarn.<sup>20</sup> With the preaching of the First Crusade, the Peace was revived in both its sanctified and instituted forms, but apart from extending the Truce to all of France, the statutes of Clermont (1095) made no institutional innovation.<sup>21</sup> Nevertheless, the pontifical effort to mobilize a Christian community in peace surely helps to explain why the synodal statutes of Rouen (1096) prescribe a general oath to the peace by all men aged twelve or over, and why this oath provides for their military aid. A nearly identical provision occurs in the legatine statutes of Auch (ca. 1140), where, as in the Norman text, it is associated with conventional definitions of the Truce and Peace such as could be found in papal legislation.<sup>22</sup> The first known efforts to institute general oaths and armies for the peace seem thus to have been connected with the disciplinary promotions of the reformed papacy, but there is no reason to suppose that these supplementary institutes were yet very common, much less commonly observed, during the earlier twelfth century. In much of the North and South the older structure of obligations, partly statutory, partly customary, continued to prevail: territoriality, engagements of the regional elites, and the independent jurisdiction and enforcement by lay authorities. These are precisely the characteristics of the remarkable "peace for the whole realm" constituted by King Louis VII at Soissons in June 1155 and sworn by the "baronage" of France. Not until then could a Capetian monarch, his vision of the old Frankish kingdom restored by the laborious pacification of its petty lordships, have realistically taken this path boldly marked out by the great dukes and counts.<sup>23</sup>

NOW AS IT HAPPENED—and surely not accidentally—Louis VII's constitution nearly coincided with a revival of the peace movement in southern France and Spain. The pontifical interest as reaffirmed by the Second Lateran Council had found local expression not only at Auch, but also in the Mâconnais and at Narbonne in the 1140s and early 1150s. In 1154 the king had made pompous journeys in Spain, where he had taken a wife and visited Compostella, and he cannot have been ignorant of efforts by the cardinal-legate Jacintus to promote the Truce and Peace in the peninsula.<sup>24</sup> Returning via Occitania, Louis

<sup>20</sup> Hoffmann, 102–03, 133–34. Only for Berry is there evidence pointing to the early establishment of a military-fiscal obligation for the peace, and even in this case it is not clear whether the custom dates back to the famous *communa* of 1037 or was established only much later: cf. Hoffmann, 105–15, and Devailly, *Berry*, 142–48, 489–91.

<sup>21</sup> *Decreta Claramontensia*, ed. Robert Somerville, *The Councils of Urban II*, 1 (Amsterdam, 1972): 73–74, 94, 103, 108, 116, 124, 143; Erdmann, *Kreuzzugsgedanken*, 311 ff.

<sup>22</sup> Albert Vermeesch, *Essai sur les origines et la signification de la commune dans le Nord de la France (XI<sup>e</sup> et XII<sup>e</sup> siècles)* (Heule: Studies presented to the International Commission for the History of Representative and Parliamentary Institutions, 30, 1966), 48–57; and see below at note 24.

<sup>23</sup> *Recueil des historiens des Gaules et de la France*, ed. Martin Bouquet et al., 24 vols. (Paris, 1738–1904), 14: 378–88; and see generally Aryeh Graboïs, "De la trêve de Dieu à la paix du roi: Étude sur les transformations du mouvement de la paix au XII<sup>e</sup> siècle," *Mélanges offerts à René Crozet . . .*, ed. Pierre Gallais and Y.-J. Riou, 2 vols. (Poitiers, 1966), 1: 585–96. Possibly, however, the Peace of Soissons had a precedent early in the reign of Louis VI: Ivo of Chartres, ep. 258 (*PL*, 162: 259), cited and well interpreted by Graboïs, 588. The latter sees the Peace of Soissons as the culmination of a half-century's efforts by Louis VI, Suger, and the popes to exploit the Truce of God to the advantage of the realm; cf. below, page 310.

<sup>24</sup> *Conciliorum oecumenicorum decreta*, ed. Joseph Alberigo et al., (3d ed., Bologna, 1973), 199 (canons 11, 12); Hoffmann, *Gottesfriede*, 121, 139; for activity at Narbonne, see below; and for the texts on Spain, Carl



had visited bishops' churches; he may have witnessed a settlement of hostilities between the viscountess and the legate-archbishop of Narbonne (January 1155), and his peace for the realm should not be dissociated from the circumstance that between 1155 and 1174 most of the bishops (and several abbots) in the province of Narbonne sought and obtained the king's protection together with his recognition of their tenure of regalian rights.<sup>25</sup> Moreover, a new fact has come to light, in a privilege for the Templars first printed in 1972, which renders these connections the more likely. On April 27, 1155 Pope Adrian IV confirmed what was described as an *institutio* of the peace and truce that had been decreed by Archbishop Arnaud I of Narbonne (1121–49) with the counsel and consent of the lay magnates of his province, but without mention of any suffragan bishops.<sup>26</sup> This enactment secured for the Templars of (apparently) the whole province a major role in peacekeeping together with a fiscal endowment pegged to that role; the king must have heard about it when he passed through, and it is hardly far-fetched to speculate that the bishops had misgivings about it. In the event, this singularly valuable privilege was to be confirmed by Adrian himself between 1157 and 1159, by Alexander III on at least five occasions between 1162 and 1176, and by Clement III in 1190, when it was extended to the county of Provence in a major assembly; nor was this the limit of its influence.<sup>27</sup> Meanwhile, institutes of peace were introduced in the city of Elne (1156),<sup>28</sup> in the heartlands of the county of Toulouse (about 1163),<sup>29</sup> in the dioceses of Comminges (1170)<sup>30</sup> and Rodez (probably 1169 or 1170),<sup>31</sup> and confirmed in the dioceses of Béziers (ca.

Erdmann, *Das Papsttum und Portugal im ersten Jahrhundert der portugiesischen Geschichte* (Berlin, 1928), 55–58; and Ferran Valls-Taberner, "Ein Konzil zur Lerida im Jahre 1155," *Papsttum und Kaisertum. Forschungen . . . Paul Kehr zum 65. Geburtstag dargebracht*, ed. Albert Brackmann (Munich, 1926), 364–68.

<sup>25</sup> *HL*, 4: 230; and texts registered by Achille Luchaire, *Études sur les actes de Louis VII* (Paris, 1885), nos. 339, 349, 355, 366, 367, 379, 387–89, 446, 456, 461, 649. There was also an important privilege for the bishop of Mende in 1161 (no. 452); among episcopal privileges, only that for Agde postdates 1161.

<sup>26</sup> *Papsturkunden für Templer und Johanniter*, ed. Rudolf Hiestand (Göttingen: *Abhandlungen der Akademie der Wissenschaften . . . Philologisch-historische Klasse*, 3d ser., no. 77, 1972), no. 27, discussed further and quoted below, page 60. Hiestand expresses the view that this was not "einen eigentlichen Gottesfrieden, sondern . . . einen besonderen Schutz für die Viehhabe und die Ackerbestellung, wie aus dem Text deutlich wird." To define the "real peace of God" in this way is surely to misunderstand the *historical* nature of the phenomenon: a spontaneous evolution of specific protections, diversely expressed, and sanctioned by lay and clerical authorities.

<sup>27</sup> *Ibid.*, no. 27 (pp. 233–34); see also no. 218, and cf. *Catalogue des actes des comtes de Toulouse*. Volume 3. *Raymond I (1149–1194)*, ed. E.-G. Léonard (Paris, 1932), no. 135.

<sup>28</sup> Bibliothèque nationale, Collection Moreau, 68: fols. 1–2, 4–5v (from lost cartulary of Elne; also in Baluze, 108: 83–84v), printed incompletely by Auguste Molinier, *HL*, 5: 1190–92; cf. cols. 1184–85.

<sup>29</sup> *HL*, 5: 1270–71, iv; cf. 1270, iii (see *Actes de Raymond V*, nos. 27–30), Count Raimond V refers to "pacem quam ego in Tolosae & in Albiae episcopatu mittam," June 1163, following his peace settlement with the viscount of Béziers.

<sup>30</sup> *Papsturkunden in Frankreich*. Volume 7. *Gasconne, Guienne und Languedoc*, ed. Wilhelm Wiederhold (Berlin: *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen, Phil.-hist. Klasse*, 1913), no. 80, papal confirmation to bishop and clergy of Comminges of protection for oxen and ox-herds; one measure of harvest-grain (*messis*) per yoke payable to Templars, 10 May (1170). Some of the background is provided by Charles Higounet, *Le comté de Comminges de ses origines à son annexion à la couronne*, 2 vols. (Toulouse, 1949), 1: 48–60.

<sup>31</sup> *Gallia christiana* . . . , 16 vols. (Paris, 1715–1849), 1: *instrumenta*, 51–52, papal confirmation of a "peace" for all property and persons established by bishop of Rodez "consilio abbatum, praepositorum et archidiaconorum tuorum et baronum terrae," together with Count Hugues II of Rodez; provision for taxation of those with churches or other property; 1166–70. See also Roger Bonnaud-Delamare, "Une bulle d'Alexandre III en faveur de la paix (1170)," *Annales du Midi*, 51 (1939): 68–86.



1170)<sup>32</sup> and Albi (1191),<sup>33</sup> in the county of Roussillon (1173),<sup>34</sup> and possibly in the diocese of Mende (ca. 1170).<sup>35</sup> A remarkable constitution for the peace in Bordelais was promulgated in the name of King Richard I between 1189 and 1195.<sup>36</sup> Across the mountains Alphonse II imposed the peace in Aragon in 1164 in the presence of prelates, including the master of the Templars, of barons, and of the deputies of six towns;<sup>37</sup> and in Catalonia, where he progressively improved his control of the Pyrenean counties, he drew on the reinstituted peace of Roussillon (mentioned above) to initiate a new series of sworn statutes of peace and truce (Fontaldara 1173, Gerona 1188, and thereafter) to apply almost everywhere in Catalonia.<sup>38</sup> In Urgel, the only major Catalan county still independent of the count-king, the institutes of 1187 were clearly influenced by the royal program.<sup>39</sup> It was in these statutes, supported by the popes, that the Catalanian peace assumed the administrative order already mentioned. And from the eastern borders of Aragon to Périgord and to Provence the movement persisted vigorously in the early thirteenth century.<sup>40</sup>

One has only to juxtapose the statutory evidence of this development to see how novel it was. For the first time, institutions specific to the Peace were routinely provided for: oaths to uphold the peace, armies of enforcement, taxation to support such armies or to compensate the victims of violence, officers to police the peace. For all of this there were precedents in the impulses of the preceding century, and the oaths and armies, at least, had occasionally figured in the old statutes; but none of these practices had been effectively secured in the instituted peace. Whereas the old oaths had been purely negative, the new ones, such as were prescribed in the statutes of Elne (1156) and Tarascon (1226), included positive commitments to serve in the peace-force and to pay the peace-tax.<sup>41</sup> Moreover, the legislators of the twelfth

<sup>32</sup> *HL*, 8: 275–76, bishop of Béziers has convoked viscount “& milites terrae . . . & pacem conjurare fecimus”; the archpriest is directed to obtain oaths of parishioners and to oblige them “ad sequendum pacem & cogendum illos, qui eam infregerint, apud Sarzac a nobis commoniti, quandocumque vocati fuerint.” Those who fail to pay Templars their due “pro pare bovum” are excluded from protection; ca. 1170. Magnou-Nortier, *Société laïque et l’église*, 526–27 and note 17, mistakes both the date and the significance of this text: the reference to the Templars’ right invalidates her date of 1128 (cf. below, page 300).

<sup>33</sup> *Gallia christiana*, 1: *inst.*, 6–7 (Léonard, *Actes de Raymond I*, no. 144). Count Raimond V and bishop of Albi, “consilio domini Rogeri Biterrensis vicecomitis, et Sicardi, vicecomitis de Lautrec, & Albiensium baronum, et illustrium virorum, in Albiensi diocesi pacem constituerunt,” for the support of which the count and bishop are to receive one *setier* of grain per plow animal and graduated payments in coin for horses or asses; 1191.

<sup>34</sup> T. N. Bisson, “Une paix peu connue pour le Roussillon (A.D. 1173),” *Droit privé et institutions régionales. Études historiques offertes à Jean Yver* (Paris, 1976), 69–76, reformulation of eleventh-century statutes of Toulouges.

<sup>35</sup> See below, page 304.

<sup>36</sup> *Cartulaire de l’église collégiale Saint-Seurin de Bordeaux . . .*, ed. J.-A. Brutails (Bordeaux, 1897), no. 204.

<sup>37</sup> *Colección de documentos inéditos del Archivo general de la Corona de Aragón*, ed. Próspero de Bofarull y Mascaró, 42 vols. (Barcelona, 1847–1973), 8: 36–41. The peace was not, however, to be so influential in Aragon as in Catalonia.

<sup>38</sup> *Cortes de Cataluña*, 1:1, 55 ff.; Bisson, “Paix pour le Roussillon,” 71–76.

<sup>39</sup> “Pau i treva del comtat d’Urgell en 1187,” ed. Ferran Valls Taberner, *Revista Jurídica de Catalunya*, 34 (1928): 354–56; Odilo Engels, *Schutzgedanke und Landesherrschaft im östlichen Pyrenäenraum (9.-13. Jahrhundert)* (Münster: *Spanische Forschungen der Görresgesellschaft*, 2d ser., 14: 1970), 267.

<sup>40</sup> *Cortes de Cataluña*, 1:1, 76–112; *ACP*, nos. 57, 102; *LTC*, 2: no. 3412; T. N. Bisson, *Assemblies and Representation in Languedoc in the Thirteenth Century* (Princeton, 1964), ch. 3.

<sup>41</sup> Cf., e.g., Hefele-Leclercq, *Conciles*, 4:2, 1409–10, with *HL*, 5: 1190–92, and with *ACP*, no. 102, art. 25. The oaths of Rouen and Auch (above, page 295) are the first known to refer to a peace-force; that of Elne

century freely widened the scope of the peace. The security of peasant property, having been recognized as a disaster-area, was made a special department of the peace in Catalonia, where it was known as the “peace of beasts” (*pax bestiarum* or *bovaticum*; Catalan: *bovatge*), and in the province of Narbonne; an old tendency to shelter coinages under the Peace was confirmed; while from the time of the Third Lateran Council (1179) the popes, in promoting their “business of the peace and faith,” encouraged the faithful to think of heretics as violators of the peace.<sup>42</sup>

Now a structure of power with these attributes looks for all the world like a public and utilitarian order answering to regional needs at the expense of privilege. In 1173 Alphonse II spoke of providing for the “public utility of all my land” when he established the peace and truce from Salses to the Tortosa-Lérida line, and his language perpetuated in secularized form the Carolingian usage of the old synodal texts.<sup>43</sup> German scholars have not hesitated to interpret such Catalonian institutes as *Landfrieden*, nor can it be doubted that these laws of the peace differed categorically from the customs of fiefs.<sup>44</sup> Yet if we ask how contemporaries regarded the newly institutionalized peace, we shall find that the Catalonian program was in one critical respect—its fiscal basis—less progressive than the diocesan program across the Pyrenees.

Toward 1205 some Catalan barons, among other complaints about royal administration, charged that the count-king had demanded payments in return for maintaining the rural peace and a stable coinage. Now this might seem on its face a negligible complaint; yet the remarkable fact is that the king’s men seem to have admitted its force, not so much by their action—for the remedial charter by which we know of these events seems not to have been promulgated—as by their language. This charter itself referred to the impositions as “ransoms” of the peace and the coinage: “nor shall I [Peter II] cause it [the coinage of Barcelona] or the *bovaticum* . . . to be redeemed.”<sup>45</sup> Was there no utilitarian claim, then, for these taxes? Evidently not, for the kings themselves were used to speaking of the *bovatge*, like the *monedatge*, as a ransom, Peter II having done so even when most desperately in need of new fiscal resources.<sup>46</sup> And, in fact, Peter was true to tradition, because we happen to know that the Catalonian levy for the peace had originated as a ransom—as a sale, in other words, not as a tax. The redemptions of coinage and of *bovatge* first appear in a charter of 1118 according to which Ramon Berenguer III,

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represents the earliest such oath certainly indigenous to the South; while the *Cartulaire de Béziers: Livre noir* . . . , ed. J.-B. Rouquette (Paris-Montpellier, 1918), no. 223, preserves an undated oath (1160–67) of the older type but associating heresy with other violations of the peace.

<sup>42</sup> T. N. Bisson, “Sur les origines du *monedatge*: quelques textes inédits,” *Annales du Midi*, 85 (1973): 91–98; Alberigo, *Conciliorum decreta*, 222, 224 (chs. 21, 22, 27).

<sup>43</sup> *Cortes de Cataluña*, 1:1, 55–56.

<sup>44</sup> The latter, to be sure, were partly codified in the *Usatges*; but even there they are perfectly distinct from regulations of the peace and truce. See, e.g., *Usatges de Barcelona*, ed. Ramon d’Abadal i Vinyals and Ferran Valls Taberner (Barcelona: *Textes de dret català*, 1: 1913), arts. 5–26, 60–66, 133.

<sup>45</sup> Ed. Bisson, “An ‘Unknown Charter’ for Catalonia,” p. 76: “Promitto similiter quod monetam barchinonensium [*sic*] in tota uita mea non mutem nec deteriorari permittam nec faciam ipsum [*sic*] uel bouaticum deinde redimi.” *Bouaticum* has here its original meaning of “peace of beasts”—the rural peace.

<sup>46</sup> Bisson, “Origines du *monedatge*,” 99; *Cortes de Cataluña*, 1:1, 84, art. 16.

upon his succession to the county of Cerdanya, instituted his peace and his coinage (of Barcelona) in that county; it was stipulated that in return for this peace and for his promise to hold the coinage stable, he was to collect for *this one time* a tribute assessed on persons, oxen, and plows.<sup>47</sup>

Now in this form the *bovatge*-levy was a better bargain for the mountaineers than for their prince. Even had the charter become a precedent, it could only have cost the knight or the peasant household a sou or two once a generation to obtain solemn guarantees of the rural peace. Yet when subsequent counts of Barcelona tried to invoke this precedent throughout their domains, they encountered the resistance of people who apparently contemplated no worse alternative. Peter II cannot have forgotten that already in 1188 his father had been obliged to renounce exactions "on the occasion of the *bovatge* or of the constituted peace from any men settled [in Catalonia]."<sup>48</sup> This personal engagement did not prevent Peter himself from imposing the *bovatge*, nor perhaps from justifying it as an accession-tax; for by that time the old charter for Cerdanya (known as *Cunctis pateat*) had very probably been incorporated in the *Usatges of Barcelona*.<sup>49</sup> But it looks as if most Catalans thought that the levy for the peace was virtually a seigniorial exaction, hardly more tolerable than the arbitrary protection-money collected by northern lords from helpless peasants.<sup>50</sup>

Why was this so? The most likely explanation is that the twelfth-century counts of Barcelona had failed to establish any cogent justification for the peace-tax. Powerful rulers committed by the early statutes to the maintenance of the peace, they retained the right to convoke the free men in times of military need, and we know from an inventory of comital domains made in 1151 that this military obligation was commutable at a stiff rate.<sup>51</sup> Nevertheless, it appears that the need for an institution reserved for peace-keeping, such as was then being devised in Occitania, was increasing in the time of Alphonse II, for in the very assembly in which he renounced the *bovatge*, that king prescribed the rural militia based on unpaid service from the manses which was to be, in fact, the principal institutional component of the reorganized Catalan peace.<sup>52</sup> In short, the peace-tax in Catalonia was premature and too severely restricted to be adaptable to the needs arising after 1150.

Meanwhile, the knights Templar in southern France had acted more cannily than Ramon Berenguer III. For it was almost certainly on their initiative that toward (probably) 1145–47 Archbishop Arnaud I of Narbonne, together with Counts Alphonse-Jourdain of Toulouse and Hugues I of Rodez, Viscount Roger of Carcassonne "and other noble men of that land," instituted a peace and truce for peasants, oxen, and other farm animals, in return for

<sup>47</sup> *Liber Feudorum Maior*, ed. Francisco Miquel Rosell, 2 vols. (Barcelona, 1945–47), 2: no. 691.

<sup>48</sup> *Cortes de Cataluña*, 1:1, 67, art. 18.

<sup>49</sup> *Usatges*, art. 172; the *terminus a quo* for this article may be set at about 1150.

<sup>50</sup> Cf. J. F. Niermeyer, *Mediae latinitatis lexicon minus* (Leiden, 1954–76), v. *salvamentum*.

<sup>51</sup> Archivo de la Corona de Aragón, Cancillería, pergaminos Ramon Berenguer IV, no. 233 (B).

<sup>52</sup> *Cortes de Cataluña*, 1:1, 66, art. 10; 69–70, art. 5. The militia of the manses, also attested in Andorra (*Privilegis i ordinacions de les valls pirenenques*, ed. Ferran Valls Taberner, 3 vols. [Barcelona: *Textes de dret català*, Volume 2, 1915–20], 3: no. 3) may have been descended from older comital authority.

which the Templars were to receive an *annual* payment of one *sétier* of grain per plow: at current local values and, counting only the first year, roughly twice the amount of the levy in Cerdanya a generation before. This constitution was to be made public in each parish and to be guaranteed by prelates and princes, who were to appoint "suitable persons" in each castle-town and village to collect the revenue.<sup>53</sup> The endowment seems to have been the extension of a similar privilege, also dating from the pontificate of Eugene III (1145–53), applied to oxen placed explicitly under the Templars' protection; the more sweeping privilege was sanctioned by Adrian IV in 1155, as we have seen, but the older one was not forgotten and was to be instituted in Comminges in 1170.<sup>54</sup> In some way, it is clear, the Templars—a specialized *militia Christi*—were being represented as guarantors of the rural peace. This was a doubly appropriate role, for we know that the maintenance of the old Peace and Truce had seriously deteriorated in coastal Languedoc and Roussillon during the second quarter of the twelfth century, and that the Templars in particular had suffered serious damage in their rapidly growing patrimonies.<sup>55</sup> Nor could the bishops effectively oppose the Templar ambitions. Among the royal privileges for southern sees issued between 1155 and 1174, only one (that for Uzès) reserved the bishop's right to proceeds of a tax for the peace,<sup>56</sup> a fact which suggests that the idea of such a tax was still uncommon when the instituted peace was revived in the Midi. The most interesting

<sup>53</sup> Hiestand, *Papsturkunden für Templer*, no. 27, to prelates, princes, and *fideles*: "... Et quidem frater noster Arnaldus Narbonensis archiepiscopus consilio et assensu illustrium uirorum A(defonsi) comitis Tolosani, Hugonis comitis Rutenensis, Rotgerii uicecomitis Biterrensis necnon aliorum nobilium uirorum illius terre pro reuerentia ac sustentatione eorundem militum hanc institutionem in suis partibus firmauerunt: boues et omnia aratoria animalia, bubulci quoque omnisque apparatus arantium animalium necnon homines et bestie, que semina uel aratrum ad campum detulerint, omni tempore sint secura ... [provisions for anathema, including interdict on communities failing to make due restitution] Pro unoquoque etiam aratro sextarium frumenti eisdem militibus annualiter persoluatur. Et quoniam nostri officii est ea, que ad pacem atque securitatem fidelium pertinent, constituere et firmare, eandem treugam atque institutionem auctoritate apostolica confirmamus et, ut eam per uestras parrochias nuntietis atque id ipsum a uestris parrochianis fieri faciatis et pariter obseruari, in peccatorum uestrorum remissionem uobis iniungimus. Studii autem uestri sit, ut per singula castella uel uillas idoneas personas ad recolligendos eosdem redditus uestro auxilio per eorundem militum dispositionem statuatur, que nimirum eandem helemosinam fideliter colligat et cum omnibus rebus suis sub eiusdem pacis et treugue Dei defensione consistat. ..."

Overlooked by the older historians, this allusion to an assembly is not perfectly clear. Roger I (d. 1150) was not viscount of Béziers, but of Carcassonne, although such a slip could easily have been made in the papal chancery several years after his death, when his brother Raimond-Trencavel of Béziers had succeeded him. An occurrence after 1148 or 1149 is rendered unlikely by the references to Archbishop Arnaud (d. 1149) and Count A(defonsus) of Toulouse, who died in the Holy Land in 1148 (although Raimond V's brother Alphonse styled himself count of Toulouse in 1154, *HL*, 4: 226). It is singular that so important an event should have left no other trace in the records, and the possibility of some misrepresentation in the privilege of 1155 cannot, perhaps, be entirely excluded.

<sup>54</sup> Wiederhold, *Papsturkunden in Frankreich*, 7, no. 80.

<sup>55</sup> Jaime Villanueva, *Viaje literario a las iglesias de España*, 22 vols. (Madrid-Valencia, 1803–52), 6: 340–41 (synod of Narbonne, to be dated ca. 1140, *HL*, 3: 717; 4: 227); *HL*, 4: 230; and 5: 1172; by his will of April 1154. Viscount Raimond-Trencavel renounced "mala quae ego feci cum mea cavalgada in Rossilono domibus Templi & domibus Hospitalis, quod homo redrecet eis ad suam mercedem, & infracciones quas ego in eadem terra ecclesiis cum eadem cabalgata, quod homo redrecet eis cum laudamento episcopi de Helna. ...". See generally *Cartulaires des Templiers de Douzens*, ed. Pierre Gérard and Élisabeth Magnou (Paris, 1965).

<sup>56</sup> *HL*, 5: 1201. In Périgord a "conuiuium pro pace observanda" payable by peasants is attributed to Bishop Guillaume de Naucars (1123–37) by the *Fragmentum de Petragoricensibus episcopis* ... , ed. Philippe Labbé, *Novae bibliothecae manuscriptorum librorum tomus primus [secundus]* ... , 2 vols. (Paris, 1657), 2: 738–39.

known precedent for the Templar tax was the imposition of one morabetin, or less according to ability to pay, levied on the faithful of the province of Narbonne by a council in 1140 to help the bishop of Elne redeem Christians captured by the Saracens in destructive raids on Roussillon.<sup>57</sup> The Templars cannot have forgotten that it was Archbishop Arnaud who had ordered this payment when they approached him a few years later with their own financial scheme for his province. But their endowment was otherwise very different from the ad hoc ransom. To all appearances, it was thus the Templars of Occitania who initiated the regime of regulated and recurrent taxation for the peace over extended territories.

How well the Templars used this revenue is more doubtful. It may have been their failure to justify the endowment, which was probably inadequate in any case, that caused the bishops together with the successors of the lay princes who had formerly approved it to resort to stronger measures between 1163 and 1191. The payment to the Templars is confirmed in the peace of Béziers (ca. 1170) but seems to bear no relation to the sworn service in the bishop's militia there specified;<sup>58</sup> by 1191 in Albigeois the annual *sétier* of grain survives only vestigially as part of a complex assessment in which the Templars no longer have a share.<sup>59</sup> It was in the peace for Rouergue instituted probably in 1169 or 1170 that the administration and function of the peace-tax, here termed *commune*, were first articulated: the levy is a kind of insurance fund, from which those who contribute are to be indemnified in case of theft or destruction.<sup>60</sup> The *compensum . . . pro pace* of Uzès presumably had the same function; and the *commune* of Bordelais (1189–95) certainly did.<sup>61</sup> The tax in

<sup>57</sup> Villanueva, *Viaje literario*, 6: 340–41; *HL*, 4: 227.

<sup>58</sup> *HL*, 8: 275–76 (partly quoted above, note 32).

<sup>59</sup> *Gallia christiana*, 1: *inst.*, 7: “. . . Ad adiutorium istius pacis, dabitur Raimundo comiti et episcopo pro animalibus cujusque aratri, sestarium annonae, et pro uno quoque saumario duodecim denarii Albiensis monetae, et sex pro asino vel asina. De animalibus vero quae signa pacis portaverint, non poterit aliquis pro debito vel fideiussione vel aliquo delicto pignorationem facere, nisi specialiter ea sibi a domino obligata fuerint.”

<sup>60</sup> *Ibid.*, *inst.*, 51 (with variants from *Recueil des historiens*, 15: 886–87): “. . . Ad hujusmodi vero pacis et securitatis sustentationem et defensionem, statutum est ut abbates, archidiaconi, archipresbyteri, monachi, canonici, priores et omnes clerici, qui proprias ecclesias regunt, milites quoque et mercatores atque burgenses, qui facultatibus abundaverint et omnes etiam homines, tam clerici quam laici, qui habuerint par boum, seu aliorum animalium, cum quibus arare possint, sive amplius habuerint, vel qui habuerint summarium, equum scilicet vel equam, mulum vel mulam, quae ad portanda onera locent, duodecim denarios ruthenenses, sive alios tantundem valentes, donent. . . . [mention of lesser graduated payments]. Commune autem istud per singulas parochias debet reddi, cum scripto unius parochianorum, quem capellanus cum consilio sui archipresbyteri, et voluntate suorum parochianorum elegerit; et in die statuta ab ipso parochiano et cum eodem scripto ad Ruthenensem ecclesiam deferatur. Quisquis autem res suas amiserit, postquam commune, sicut praedictum est, solverit, in integrum restituatur; si tamen certam personam, quae res sibi ablatas habeat, vel locum ubi sunt poterit denunciare, sin autem minime. Si vero inimicos villas vel oppida depraedari, vel diruere forte contigerit, res quidem mobiles emendabuntur de communi, sed damna rerum immobilium non restituentur, nisi quantum a malefactoribus poterit recuperari. Clerici vero qui proprias ecclesias non habent, nisi par boum habuerint, non cogantur dare, si nolint; sed non dato communi, si forte res suas perdiderint, eis nequaquam emendabuntur. . . .”

<sup>61</sup> *HL*, 5: 1201; Archives départementales de la Gironde, G 1030, fol. 75v (Brutails, *Cartulaire de Saint-Saurin*, no. 204): “. . . Pro vero pace tenenda dederunt domino regi commune barones et omnes alie gentes usque ad septem annos, primo tamen anno senescallie G. de Cellis non computato propter paupertatem gentis; sed a [festivitate] sancti Michaelis que erit in secundo anno senescallie, reddetur commune per septem sequentes annos. Commune vero tale est: quodlibet par boum cum bubulco reddet .xii. denarios; similiter villanus, si sibi bubulcus fuerit et alium bubulcum non habuerit, reddet .xii. denarios pro se et



Albigeois was vaguely termed “aid of the peace” in 1191, but we know that in Quercy the proceeds of such levies were being used from about this time to pay wages to fighting men as well as amends,<sup>62</sup> and it was a tax of precisely this description that became customary in Albigeois and was legislated in the legatine council of Montpellier in 1215,<sup>63</sup> whence it passed to Provence.<sup>64</sup>

Accordingly, these fiscal obligations, unlike the *bovatge*, could be defended on utilitarian grounds. They resembled the tallage then being instituted in many towns for wall-building, paving, and the like: for example, the levy on property for the fortification of Le Caylar (near Lodève) in 1158, from which “proctors for the public work” were to be supported for five years.<sup>65</sup> But the diocesan peace was organized more comprehensively and less definitely than the urban utilities: it required certain annual payments for uncertain contingencies and made no effective provision for responsibility or accountability. The results were not everywhere satisfactory. Exemptions from the levy were being granted as early as 1199 in Rouergue.<sup>66</sup> In Périgord the *commune*, although still associated with the peace, was already defined as a customary hearth tax when Capetian authority was recognized at Puy-Saint-Front in 1246.<sup>67</sup> In Albigeois the men of Gaillac complained in 1252 that the funds had been misappropriated and that the tax was unjustified in times of peace and order. Their allusion to the maxim *cessante causa debuit cessare effectus*, while surely tendentious, implied their inability to conceive of the maintenance of order as a public necessity.<sup>68</sup> It continued to be harder to institute offices than obligations.

Meanwhile, events had taken a curiously different turn in Catalonia. There the count-kings managed to convert the old and unpopular ransom of the

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bobus suis. Omnis homo duodecim annorum et supra qui possit habere viginti solidos de mobili reddet .xii. denarios; si vero non possit, reddet .vi. denarios, si habeat unde possit . . . [lesser graduated assessments] . . . Item, qui commune domini regis recipiet faciet indici per parrochias ut ad septem dies persolvatur; . . . Item si aliquis illorum qui commune reddiderit aliquid perdet quod per domini regis iusticiam non reddatur, de communi domini regis ei emendabitur, ut tamen habere possit duos legitimos homines et cappellanum testes sui dampni. . . .”

<sup>62</sup> See quotation in note 59; Archives nationales, J 896, no. 33, ed. Edmond Albe, *Cahors: Inventaire . . . des Archives municipales. Première partie, XIII<sup>e</sup> siècle (1200–1300)* (Cahors, n.d. [1915]), results of a royal *enquête* dating from the 1250s, e.g., p. 46: “1. Prior Caturcen., juratus et interrogatus de levatione communis in Caturcino, dixit quod ipse, tempore episcopi Geraldi qui decessit quadraginta anni sunt et amplius, vidi bis commune levare, dicens quod pro pace servanda in diocesi levabatur, auctoritate episcopi, cum voluntate baronum et magnarum villarum, et inde fiebant emende et dabantur stipendia militatibus . . . Dixit etiam quando aliquid erat residuum de communi episcopus inde retinebat et distribuebat inter barones et pacificatores sive paciarios [Albe mistakenly reads *pactarios*].”

<sup>63</sup> *HL*, 8: 1311; Mansi, 22: 948, ch. 39: “Si pax fracta fuerit ab iis qui sunt intra terminos pacis, debet pax fracturam pacis quaerere utroquo gladio; & si non poterit recuperare, debet emendare de compenso vel de nova collecta: de quo compenso, seu nova collecta, si compensum non sufficit, debent expensae fieri equitibus qui cum armis pacem sequuntur.” The counts of Toulouse insisted on a half-share of the *compensum pacis* in the dioceses of Viviers (1215), Agde (1224) and Albi, *HL*, 8: 665, 802, 1310–12.

<sup>64</sup> *ACP*, no. 57, art. 9; no. 102, arts. 7, 27.

<sup>65</sup> *LTC*, i, no. 150.

<sup>66</sup> *Documents sur l'ancien hôpital d'Aubrac*, ed. J.-L. Rigal and P.-A. Verlaquet, 2 vols. (Rodez: *Archives historiques du Rouergue*, 4, 11; 1913–17), 1: no. 12.

<sup>67</sup> *LTC*, 2: no. 3412. It seems likely that in Périgord as elsewhere the peace was organized in the later twelfth century, although the possibility remains that the *commune* was descended directly from the early twelfth-century *conuiuium* (see above, note 56).

<sup>68</sup> *HL*, 8: 1310–12; Bisson, *Assemblies in Languedoc*, 110.



peace into the first statewide subsidy in their history. Peter II not only ignored the draft-charter of 1205 but increased the rates of assessment in the levies to secure the peace and the coinage.<sup>69</sup> Whether he violated the principle of redemption-upon-accession is less clear, for we have no proof in extant texts that the same men had to pay these impositions more than once during his reign. Peter certainly imposed the *bovatge* in the diocese of Vich upon his accession: a remarkable summary of account dating from 1200 shows proceeds amounting to nearly 20,000 sous.<sup>70</sup> In 1210 a *bovatge* in Cerdanya and Roussillon netted more than five times that much, and other *bovatges* were levied or contemplated in Peter's last years.<sup>71</sup>

Now it cannot be ruled out that some part of these impositions was devoted to peace-keeping. The principle of redemption persisted, as we have seen, and if the vicars were paid salaries, the *bovatge* would have seemed an obvious source of support. But the diocesan peace-army was not paid as such, and Peter's more urgent needs undoubtedly lay elsewhere. He not only inherited choking debts from his father but lost no time committing himself to expensive enterprises in war and diplomacy.<sup>72</sup> During the first decade of his reign, the redemptions were his major source of extraordinary revenue, and his only taxes at all comprehensive in incidence. In 1197 the bishop and chapter of Vich received a charter of nonprejudice for their contribution to the "redemption of the coinage" which the king had collected in that town "on account of the urgent necessity of the Saracen army" (that is, of the Christian campaign to avenge Alarcos).<sup>73</sup> It looks as if the promise to secure the coinage no longer sufficed to justify the levy; negotiation was now required and with the appeal to "urgent necessity" the nature of the payment was profoundly altered. Almost certainly the same or a similar ulterior urgency motivated the collection of *bovatge* that was accounted in 1200. Moreover, a *bovatge* in 1211 was occasioned by the crusade that was to culminate in Las Navas de Tolosa, according to a lost text cited by Zurita; while in 1210 the cattle-tax was identified or confused with the money-tax in Cerdanya (as it had been almost a century before), that is, with an imposition that was now certainly a subsidy.<sup>74</sup>

So it happened that in Catalonia the purchase of peace became a tax for war. In a sense, this change was consistent with the enlarged ecclesiastical conception of the "business of the peace and the faith," but it is unlikely that

<sup>69</sup> For the money-tax, see Geronimo Zurita, *Anales de la Corona de Aragón* (1st ed., 1578–85), book 2, ch. 52; ed. Antonio Ubieto Arteta and Maria D. Pérez Soler, 3 vols. in 4 to date (Valencia, 1967–72), 2: 144–45; for the *bovatge*, citations in notes 70, 71. Analysis of the *bovatge* accounted in 1210 suggests that either the strictly agrarian assessment (on animals and tools) was increased over that of 1118, or, more likely, a new and more comprehensive property assessment (such as Zurita mentions for the money-tax) was devised to tap urban prosperity.

<sup>70</sup> Archivo capitular, Vich, mensa episcopal, register "Variarum rerum," no. 35.

<sup>71</sup> Bisson, "Origines du monedatge," 101–04 (nos. 3, 4); Zurita, *Anales*, book 2, ch. 69 (ed. Ubieto, 2: 210).

<sup>72</sup> Próspero de Bofarull y Mascaró, *Los condes de Barcelona vindicados . . .*, 2 vols. (Barcelona, 1836), 2: 216–27; Soldevila, *Història de Catalunya*, 217–30.

<sup>73</sup> Bisson, "Origines du monedatge," 99–100 (no. 1); Zurita, *Anales*, book 2, ch. 48 (ed. Ubieto, 2: 132).

<sup>74</sup> Citations in note 71.

this coherence was so well grasped in Catalonia as it would have been in Languedoc, where the papal agents were more deeply engaged in the struggle against heresy.<sup>76</sup> Whatever the disposition of its proceeds, the *bovatge* was not to attain a place in the institutionalized Catalonian peace, being sanctioned only—and ambiguously—by the *usatge Cunctis pateat*. The original features of the Catalonian program were, therefore, limited to the diocesan army and the vicarial jurisdiction; and both of these institutions had evolved easily from older regalian practices in Catalonia.

IN FACT, FOR ALL THE IMPROVEMENTS WROUGHT by Alphonse II, the Catalonian peace retained its traditional prescriptions and obligations more completely than the organized peace north of the Pyrenees: the old schedule of truce-days, restitution by convicted violators without provision for a compensatory fund, and the reservation of episcopal, baronial, and royal jurisdiction. On this last point the institutes of Bordelais and Provence were most nearly comparable; it was in the more central and southern Occitan dioceses less firmly governed that appointed men usually called *paciarii* formed new courts as well as militias, and this institution was to be adopted in Provence as a last resort in case of the failure of other jurisdictions.<sup>76</sup> Although it is likely that the princes of Bordelais, Languedoc, and Provence swore to uphold the peace in their founding assemblies, as the king and the barons certainly did in Catalonia, it was only in Languedoc and Provence, so far as we surely know, that oaths were required from all men of legal age,<sup>77</sup> creating structures of allegiance irrelevant or antithetical to the abrasive groupings of petty lordships. Conceivably, the common oaths should be connected with the old sanctified peace which, having never affected Catalonia, resurfaced in upper Occitania around the shrines of Rocamadour and Saint-Privat in the third quarter of the twelfth century,<sup>78</sup> and had a famous renewal in Velay after the poor carpenter Durand's vision of the Virgin Mary in 1182. In the case of Gévaudan, where the miraculous invention of Saint-Privat about 1170 was clearly associated with local conflict, we may have an exceptional example of institutes founded or confirmed in enthusiasm. By the "golden bull" of 1161 Bishop Aldebert III (1151–87) had obtained the king's confirmation of his regalian authority;<sup>79</sup> no other statutes of peace for the diocese of Mende are extant; and while there is scattered evidence of earlier associative activity, it looks as if the measures of episcopal pacification so well attested in the thirteenth century derived chiefly

<sup>76</sup> See M.-H. Vicaire, "L'Affaire de paix et de foi' du Midi de la France (1203–15)," *Paix de Dieu et guerre sainte en Languedoc*, 102–27.

<sup>76</sup> See further below, pages 306–07.

<sup>77</sup> Mansi, 22: 948, art. 37; *ACP*, no. 57, arts. 1, 2; no. 102, art. 1; cf. Vermeesch, *Origines de la commune*, 52. There is no evidence that the oaths of Elne were imposed on men outside the city.

<sup>78</sup> See *Les Miracles de Notre-Dame de Roc-Amadour au XII<sup>e</sup> siècle*, ed. Edmond Albe (Paris, n.d.), 281–84; cf. *Chronique de Robert de Torigni* . . . , ed. Léopold Delisle, 2 vols. (Rouen, 1872–73), 2: 23–24; and below, note 88. Also *Les Miracles de Saint Privat* . . . , ed. Clovis Brunel (Paris, 1912), 132–33; cf. xxxii–xxxvi, and 105.

<sup>79</sup> *LTC*, 1: no. 168; *Chronicon breve de gestis Aldeberti*, ed. Brunel, *Miracles de Saint Privat*, 133; and for the setting, P.-A. Sigal, "Le culte des reliques en Gévaudan aux XI<sup>e</sup> et XII<sup>e</sup> siècles," *Cèvennes et Gévaudan* . . . (Mende: Fédération Historique du Languedoc . . . , 1974), 103–15.

from the energetic program of Aldebert III. The better known case of 1182 in Velay reveals a characteristically enhanced potential for organization, but also the dangers of the sanctified peace. Within a year or so sworn collectives careless of seigneurial prerogatives were spreading to neighboring regions, sprouting uniforms and taxes, but rapidly losing the support of constituted authorities who alone could have secured their effective and lasting influence.<sup>80</sup> The statutes of Montpellier (1215) perfectly evoke the seigneurial reaction, prohibiting “conjurations and conspiracies,” and permitting only such urban confraternities as are approved by lords or bishops “for urgent necessity or evident utility”—these in a provision immediately following the institutes of peace.<sup>81</sup> Associations of the countrysides, suspect as shelters for heretics or brigands, were more often the target than the creation of the approved oaths of peace.

This is one reason for doubting whether the human structures resulting from the statutory oaths in Languedoc were full-fledged associations. For Roger Bonnaud-Delamare, who emphatically rejected the idea of organizations bound by oaths, the “peace” (*pax*) of the Occitan texts was simply the ultralitururgical program of the Church, the “state of peace” which could be enjoined, constituted, accepted, or violated—could even, in Bonnaud’s view, help lords against rebellious vassals. Being cast out of the peace (*extra pacem*) upon refusal to swear the peace could not refer to an association, as Georges Molinié had thought, because in that case a pre-existent community for which we have otherwise no evidence would have to be postulated. Like the taxes and peace-keepers, the oaths were an institution of the peace but not of an association.<sup>82</sup>

This conclusion is generally supported by the texts, including the statutes of Bordelais and Provence which Bonnaud-Delamare ignored. The statutes of Tarascon (1226) include the form of the *sacramentum pacis*, which makes clear that the approved ceremony was not a social compact but an individual commitment, however it might be multiplied.<sup>83</sup> This was, of course, one of the oldest customs of the Peace of God, in respect to which the only formal innovation in the institutes of Auch, Elne, Béziers, etc., had been to make specific or written provision. The peace remained in some sense the object of personal fidelity, such as kings had been in the past; this is why the device REX had given way to PAX (or PAS) on coins minted in Béarn, Toulouse, Rodez, and possibly Roussillon from the end of the eleventh century, and on an obol minted as late as about 1215 in the name of Simon de Montfort, for whose subjects, encouraged by the prelates, the notion evidently retained its force.<sup>84</sup>

<sup>80</sup> Geoffroy de Vigeois, *Chronique*, in *Recueil des historiens*, 18: 219; *Chronicon anonymum Laudunense* in *ibid.*, 705; *III.*, 6: 106–09.

<sup>81</sup> Mansi, 22: 949–50, art. 45. The prohibition was to be reiterated in later statutes, *ACP*, no. 57, art. 5; no. 102, art. 14; Mansi, 23: 203, art. 38.

<sup>82</sup> Bonnaud-Delamare, “Légende des associations de la paix,” *Bull. phil. et historique*, 1936–37, 47–65.

<sup>83</sup> *ACP*, no. 102, art. 25.

<sup>84</sup> See Adrien Blanchet and Adolphe Dieudonné, *Manuel de numismatique française*, 4 vols. (Paris, 1912–36), 4: 32, 90, 226, 235, 350; see also J.-F. Lemarignier, in (Ferdinand Lot and Robert Fawtier) *Histoire des*

But the mechanisms of vigilance and extra-judicial constraint, about which Bonnaud had less to say, were unmistakably associative in some ways, and were among the more original elements of the organized Occitan peace. The parishioners of Béziers had not only to swear the peace but to be prepared upon summons to enforce it. Although rarely attested in the twelfth-century statutes, similar militias must have been known in most of the dioceses of Occitania: we catch glimpses of them in the Toulousain (before 1200), in Comminges (1203), then in Quercy, Agenais, Gévaudan, Velay, and (perhaps) Vivarais in subsequent thirteenth-century texts which point to traditional usage; in Agenais the episcopal militia was known as the *communia*.<sup>85</sup> The best evidence comes from Gévaudan, where the *enquêtes* beginning in 1269 produced a mass of testimony relating to the organized peace before it was superseded by the royal government. There the popular element of the peace-army was known as the *pax*, an unambiguous fact which undermines Bonnaud-Delamare's otherworldly rendering of that word in like contexts.<sup>86</sup> It can no longer be maintained that the *pax* which was to aid lords against insurgent vassals was solely spiritual; moreover, the formulary oath in the statutes of Tarascon speaks of the obligatory corps of enforcement as the peace to be "followed," an expression (*sequi pacem*) already employed in the directive of Béziers (ca. 1170).<sup>87</sup> If the peace was not always quite an association, fidelity to it unquestionably entailed an associative obligation as a last resort;<sup>88</sup> and the *paciarii* who swore "faithfully to administer the peace and all that pertains to the peace"<sup>89</sup> can hardly have avoided thinking of themselves as custodians of a communal interest. First attested in the early thirteenth century, their office (as it was called in Gévaudan) was to receive complaints, to warn violators to make amends, and, if necessary, to summon the army of enforcement. By 1210 they had become instruments of the conciliar program of pacification in lower Languedoc; in August 1214 the council of Lérida made detailed provision for their popular election with episcopal consent in the cities of Catalonia; and in January 1215 the council of Montpellier defined

*institutions françaises au moyen âge*, 3 vols. to date (Paris, 1957-62), 3: 51-52; and cf. Bonnaud-Delamare, "Bulle d'Alexandre III," 77, 80-81, who suggests that the PAS of the deniers of Rodez was a trinitarian motto derived from the exegesis of Atto of Vercelli and Rufinus (*PL*, 134: 554-55; 150: 1594): P(ater) A(lpha) X = Spiritus sanctus; hence X becomes S).

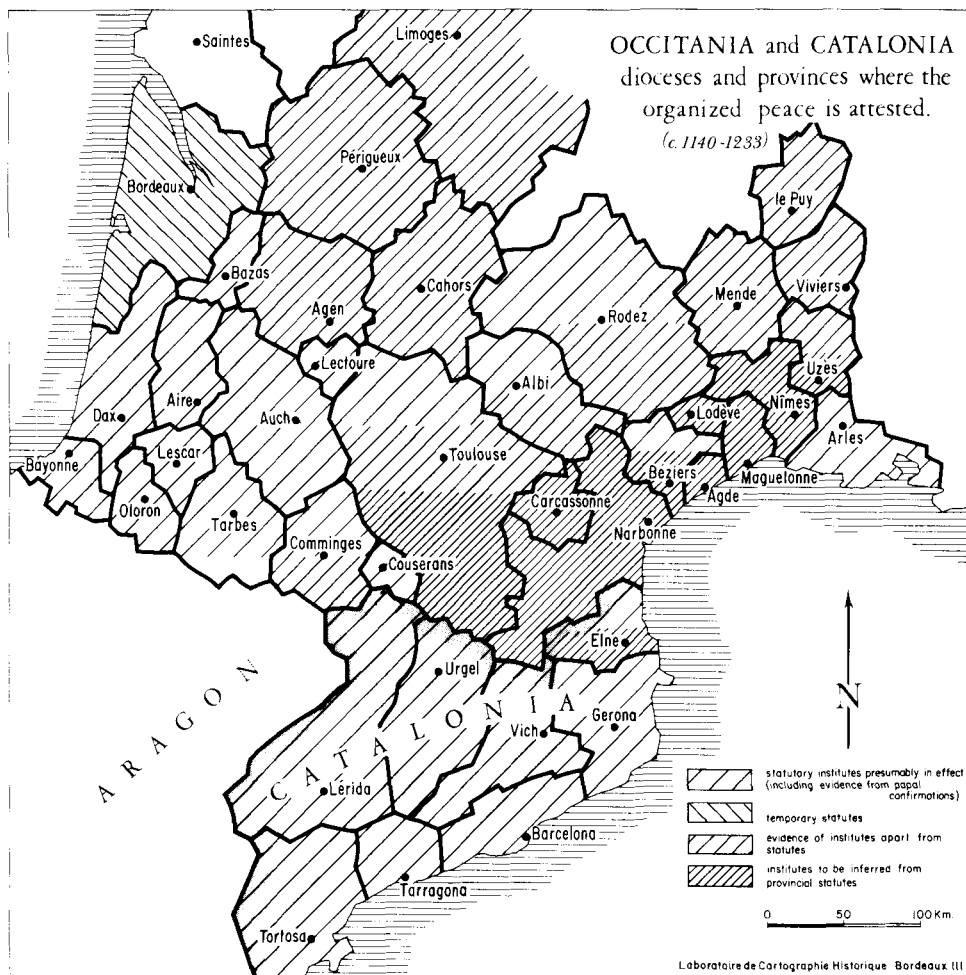
<sup>85</sup> Bisson, *Assemblies in Languedoc*, 103, 111-14, 125, 132-34. To evidence cited there, add *LTC*, 5: no. 135, a concord involving knights of Villemur-sur-Tarn (arr. Toulouse): "... Violencia nulla facta sit habitantibus infra terminum, nisi pro Treva Dei et pace ejusdem treve et pacis . . ."

<sup>86</sup> Bisson, 112-16 (based chiefly on Arch. nat., J 894, no. 9); cf. Bonnaud-Delamare, "Légende des associations," 52-53. May not the *pax* at Villemur (preceding note) also have been a militia?

<sup>87</sup> Mansi, 22: 948-49, arts. 39, 40; *ACP*, no. 102, arts. 19, 25; cf. above, notes 32, 63.

<sup>88</sup> *ACP*, no. 102, arts. 5, 25. In at least one remarkable instance, the "paci confederatio et amoris concordia et reformatio" sworn at Rocamadour by the barons of Quercy and by deputy consuls of Cahors and Figeac in February 1233 (Archives municipales de Cahors, AA 1), there can be no doubt that a palpable association was created. Obligated to mutual aid and defense for eight years, this confederation was analogous to the temporary statutes for Albi and Bordelais (and Soissons), which are not explicitly called associations; but its immediate ancestry should probably be sought in the pacts of security or concord such as were spreading among commercial towns of the Garonne valley, e.g., *Archives municipales d'Agen. Chartes. Première série (1189-1328)*, ed. Adolphe Magen and G.-E. Tholin (Villeneuve-sur-Lot, 1876), nos. 14-16, 18, 26; cf. nos. 20, 21.

<sup>89</sup> *ACP*, no. 102, art. 26.



their function in articles that were to be adopted by Raimond Bérenger V in Provence.<sup>90</sup> According to the statutes of Montpellier and Provence, the *majores paciarii* were to hold public sessions or hearings annually during the first days of May.<sup>91</sup> In Provence these “greater peace-keepers” were identified in 1226 as the count for the “whole county” and the bishops for the dioceses, together with two or three barons chosen by each bishop with the count’s consent; the structure must have been substantially the same in Languedoc, at least after 1215.<sup>92</sup>

<sup>90</sup> The first dated allusions to *paciarii* (or *paziers*) seem to be about 1210–11, *LTC*, 1: no. 935; Guillaume de Tudèle, *La Chanson de la croisade albigeoise*, ed. Eugène Martin-Chabot, 3 vols. (Paris, 1931–61), 1: 150; but they may have been known earlier in Quercy and Gévaudan, Bisson, *Assemblies in Languedoc*, 113–15, 125. See also *Cortes de Cataluña*, 1:1, 92, art. 9; Mansi, 22: 947–49, arts. 33, 34, 36, 40, 42; *ACP*, no. 57, arts. 1, 6, 7; no. 102, arts. 2, 5, 6, 8, 10, 23, 25, 26, 29.

<sup>91</sup> Mansi, 22: 949, art. 42: “Statuimus insuper, ut singulis annis in principio Maii convenient majores paciarii, & querimonias pacis expediant; & si aliquis articulus dubitationis occurrat, illum prout viderint expedire declarent.” Cf. *ACP*, no. 102, art. 10.

<sup>92</sup> *ACP*, no. 102, art. 28: “Dominus vero comes Provincie in toto comitatu suo et terra sua major est paciarius constitutus; et in singulis episcopatibus, archiepiscopus vel episcopus locorum, cum uno vel cum



THE CIRCUMSTANCES OF THE ALBIGENSIAN WAR enabled the pope, or more exactly, his legates to assume direction of the organized peace. As early as 1208 Pierre de Castelnau had secured Peter II's participation in a conjuration "of peace" against the heretics by the magnates of eastern Occitania.<sup>93</sup> The councils of Lérida and Montpellier were both led by the astute cardinal-legate Petrus Beneventanus who, finding James I (1213–76) a child in tutelage and Raimond VI, the counts of Comminges and Foix, and Simon de Montfort all appealing to the pope, was tempted to impose independent ecclesiastical mechanisms of authority from the Rhone to the Ebro. This is surely why some institutions hitherto known only in Languedoc—not only the *paciarii* but also the general oath, now carefully distinguished from the conspiratorial oath—were legislated at Lérida in 1214.<sup>94</sup> The interest of the Holy See derived, of course, from long-standing advocacy of the Peace and Truce. But it should not be imagined that the institutes promulgated in 1214 and 1215 were invented by the popes any more than were those of the preceding generation. As prescribed in the Second and Third Lateran Councils the pontifical program of the Peace was limited to defining the truce and enjoining bishops to help one another, and as late as 1195 the prelates of Narbonne province who assembled with the legate Michael, while confirming the peace "formerly sworn by will of the lord count of Toulouse," simply reiterated the old general precepts.<sup>95</sup> It seems to have been between then and the outbreak of war in 1209, and without apparent urgings from the papacy, that the taxation pioneered by certain bishops and the Templars became general in the dioceses of upper and western Occitania. In the province of Narbonne the program of 1215 may be regarded as the extension and elaboration of the peace first instituted by the archbishop and the princes toward 1145, and while the legates continued to insist on this program in the Midi, they made no effort to impose it elsewhere.

Probably they recognized its practical limitations. Heresy aside, the kinds of violence to which the institutes were opposed were like a wind-blown grass fire. Everything depended on effective local initiatives, and not even these could prevent the ravages of needy or feuding knights or mercenaries. The Brabançons had already done their damage in Limousin when at Easter 1192

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duobus de baronibus, quos ipse deputaverit ad hoc eligendos, requisita comitis voluntate." This system was slightly modified from the article of 1222, no. 57, art. 7: "Sint autem paciarii in singulis episcopatibus episcopus et unus de baronibus episcopatus, arbitrio ipsius comitis et episcopi eligendus." That no such article was included in the statutes of Montpellier points to de facto episcopal control in 1215, when the problem of higher secular authority was still unresolved in Languedoc.

<sup>93</sup> *Petri Vallium Sarnai monachi hystoria albigensis*, ed. Pascal Guébin and Ernest Lyon, 3 vols. (Paris, 1926–39), 1: 30 and note 1. In 1211 the convention providing for the consuls of Montpellier to swear to the lord-bishop of Maguelone to maintain "pacem et pacis statuta" was confirmed by the legates, *LTC*, 1: no. 960. For the general situation, see Élie Griffe, *Le Languedoc cathare de 1190 à 1210* (Paris, 1971), part 2.

<sup>94</sup> *Cortes de Cataluña*, 1:1, 90–95, esp. art. 9. On the assembly of Lérida, see Ferran Soldevila, *Els primers temps de Jaume I* (Barcelona, 1968), 67–84.

<sup>95</sup> Mansi, 22: 667–71; cf. Alberigo, *Conciliorum decreta*, 199–200, 221–22, 224–25. The council of 1195 alluded to a recent confirmation in the presence of the legate Michael at Saint-Gilles of the "pax in tota Narbonensi provincia" formerly sworn by the count of Toulouse, an act not catalogued as such by Léonard, *Actes de Raymond V*, and possibly other than the *institutio pacis* of ca. 1145–47.



an aroused populace led by the bishop and viscount of Limoges routed them in battle.<sup>96</sup> Few such direct accounts have survived for Occitania and Catalonia; and while the narrative sources clearly suggest that the proliferation of remedial institutes answered in reality to worsening conditions toward 1165–70 and 1208–14,<sup>97</sup> there is no hard evidence to show how regularly they were invoked or administered. The retrospective testimony for Gévaudan and Quercy does prove recurrent activity, but in both regions supplementary measures were required by the 1230s. Everywhere the greater aristocracy felt threatened. The Catalanian program had had to be trimmed in the time of Peter II to meet the objections of the barons whose freedom it curtailed,<sup>98</sup> and the legate Petrus surely heard protests at Lérida in 1214. It had required a sort of conceptual mutation—and dissociation from the peace!—to reinterpret the *bovatge* as justified by public necessity. In Languedoc, by a process just the reverse of what happened in Catalonia, a tax that had originated as a communal utility was threatening to become a seigneurial exaction. Everywhere the reinstituted peace retained traces of an older clerical inspiration, the “business of Jesus Christ, who is the author of peace,” as the prelates put it in 1215.<sup>99</sup> In church law the decretalist title *De treuga et pace* remained limited to the general precepts of Innocent II and Alexander III;<sup>100</sup> nor did the Catalan-born canonist Ramon de Penyafort in his own writings think to include obligations of the peace among the impositions for public utility. It was the crusade and national war, not the peace, that would entice political thought across the threshold of a new world.

NEVERTHELESS, THE MEN OF THE ORGANIZED PEACE built well. Their sworn statutes, themselves derived from private concords, furnished the procedural model for some forms of territorial legislation in the thirteenth century, notably for the statutes of Pamiers in 1212, perhaps also, via the northern experience, for contemporary Capetian *établissements*.<sup>101</sup> Their assemblies, very unlike feudal courts in this respect, were characteristically representative of the land, which thus retained its public and political identity;<sup>102</sup> and it was

<sup>96</sup> *Chroniques de Saint-Martial de Limoges*, ed. Henri Duplès-Agier (Paris, 1874), 190; see also 193, a similar incident in 1203. The continuing insecurity of church property in the province of Narbonne in the 1180s is shown by *Epistolae pontificum Romanorum ineditae*, ed. Samuel Loewenfeld (Leipzig, 1885), nos. 368, 377.

<sup>97</sup> The main texts are cited in *Assemblies in Languedoc*, 102–03. For the earlier troubles in Narbonnais and Roussillon, see above, page 300, and for a good account of problems in a neighboring diocese, see André Castaldo, *L'église d'Agde (X<sup>e</sup>–XIII<sup>e</sup> siècle)* (Paris, 1970), 57–94, 156–71.

<sup>98</sup> This is evident not only from the draft-charter of 1205 but also from new articles in the statutes of peace, e.g., *Cortes de Catalunya*, 1:1, 80 art. 7, 82 art. 11, 84 art. 16, 86–87 arts. 1, 2, 5.

<sup>99</sup> Mansi, 22: 947, art. 32.

<sup>100</sup> *Decretales Gregorii IX*, 1:34; cf. ordinary gloss (as in ed. Lyon, 1553, 268), and Damasus, *Summa decretalium*, Rome, Biblioteca Casanat., MS. 1910, fol. 76v. Canonist discussion tended to focus on the problem of just war in relation to the Peace and Truce, Hoffmann, *Gottesfriede*, 231–43.

<sup>101</sup> Cf. *HL*, 8: 625–35, with *Cortes de Catalunya*, 1:1, 55–61 (Fontaldara, 1173) with respect to solemn diplomatic forms and the confirmatory oaths; these procedural forms persist in subsequent Catalanian statutes and probably influenced those of Peter II's nephew Raimond-Bérenger V in Provence. But the nature of such relationships, in the North as well as the South, is in need of further study.

<sup>102</sup> See, e.g., the charter *Cunctis pateat* (1118), *Liber Feudorum Maior*, 2: no. 691: Count Ramon Berenguer III and the bishop of Elne, “consilio et iussione magnatum et militum totius comitatus Cerritanensis atque Confluentis, mittimus pacem in predicto comitatu . . .”; and the territorial assembly persists not only in

almost exclusively to *corts* of the peace—first in Aragon, then in Quercy, Catalonia, and probably elsewhere in the South—that the “good men” of towns and villages first came to be summoned to assemblies thereby rendered more socially representative, assemblies in which the men of the great estates of society had their first parliamentary experience.<sup>103</sup> Already before 1200 the *plena curia*<sup>104</sup> of Catalonia had become the customary occasion for the issuance of statutes of the peace and truce, a legislative tradition which was long to persist in the mature *Corts* of the later Middle Ages. The vicars retained charge of a higher royal jurisdiction that continued to administer the statutory and customary peace;<sup>105</sup> and the introduction of peace-keepers (*paciarii*) from Languedoc in 1214 was to have some influence on the organizing of elected urban magistracies in Catalonia.<sup>106</sup>

In France, on the other hand, the great peace of 1155 was not renewed. It expired at a time when lay jurisdictions were developing a new theory of high justice<sup>107</sup> and were coming into conflict with the courts Christian. The old association of lay and clerical authorities in the remedy of disorder could not be maintained where higher secular powers claimed a monopoly of regalian rights. Even so, Philip Augustus could grant or renew charters of urban peace that were, in their way, quite as territorial and nearly as institutionalized as any southern statutes.<sup>108</sup> And his royal descendants would profit from the institutes of peace that they liquidated—or rather, resumed—as they came to govern in the South. For it was the bishops, notably in Gévaudan and Quercy, who had exploited the peace to rebuild the regalian power now reclaimed by

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Catalonia, as shown by the statutes of 1173 and after, but also in Occitania, as shown by the more fragmentary records of 1145–95 often cited above.

<sup>103</sup> Bofarull, *Documentos inéditos*, 8: 36–41; Bisson, *Assemblies in Languedoc*, 125; cf. 103, 114–15, 133–34; *Cortes de Cataluña*, 1:1, 90–95, plus the list (now Archivo de la Corona de Aragón, pergamino Extra-inventario 3131) of those attending the *cort* of Lérida, 1214, reprinted from Joaquim Miret y Sans’ transcription by Soldevila, *Primers temps de Jaume I*, 83–84; and generally E. S. Procter, “The Development of the Catalan *Corts* in the Thirteenth Century,” *Homenatge a Antoni Rubió i Lluch. Miscel·lània . . .*, 3 vols. (Barcelona, 1936), 3: 528–36.

<sup>104</sup> For early evidence of this term, see *Marca hispánica*, app., no. 431; *Liber Feudorum Maior*, 1: nos. 225, 416; 2: no. 681; *Cortes de Cataluña*, 1:1, 62–63.

<sup>105</sup> This was confirmed in 1214, when the vicar’s function was redefined (*Cortes*, 1:1, 92, art. 9), making him the king’s coadjutor of the newly-constituted *paciarii*.

<sup>106</sup> See J. M. Font Rius, *Orígenes del régimen municipal de Cataluña* (Madrid, 1946), 316–25, 479. The municipal government in some places, notably Lérida and Tortosa, was later known as *paeria*. The convention of 1211 at Montpellier, *LTC*, 1: no. 960 (see above, note 93) suggests that the consuls there were already *paciarii* in all but name. On the other hand, the delayed institution of consulates in Albigeois, Quercy, and Gévaudan may have been related to the strength of the organized peace as well as to the retarded influence of Roman law: cf. André Gouron, “Diffusion des consulats méridionaux et expansion du droit romain aux XII<sup>e</sup> et XIII<sup>e</sup> siècles,” *Bibl. Ec. Charles*, 121 (1963): 26–76.

<sup>107</sup> Or repressive justice; cf. A. C. F. Koch, “L’origine de la haute et de la moyenne justice dans l’ouest et le nord de la France,” *Revue d’Histoire du Droit*, 31 (1953): 420–58, with the critique by Bernard Guenée, *Tribunaux et gens de justice dans le bailliage de Senlis . . .* (Paris, 1963), 79–81.

<sup>108</sup> Ch. Petit-Dutaillis, *Les communes françaises. Caractères et évolution des origines au XVIII<sup>e</sup> siècle* (Paris, 1947), 83–103; and see also Paul Viollet, *Droit public. Histoire des institutions politiques et administratives de la France*, 3 vols. (Paris, 1890–1903), 3: 55–56; Kennelly, “Medieval Towns and the Peace of God,” 44–53; and Vermeesch, *Origines de la commune*. In Burgundy the peace sworn by the magnates in 1153 was renewed at least once in later years, *Cartulaire de Saint-Vincent de Mâcon . . .*, ed. M.-C. Ragut (Mâcon, 1864), cclviii–cclix; *PL*, 200: 249; in Berry, where the precedents had been very strong, the archbishop’s peace, including a sworn commitment to “follow the *communia*,” persisted in the thirteenth century, Hoffman, *Gottesfriede*, 106–15; Devailly, *Berry*, 490, but cf. 381–445, and see also next note.

the king.<sup>109</sup> The organized peace was the dominant if not quite the only coercive structure in much of Languedoc during the troubled generation of the Albigensian crusades. Firmly continuous with the novel impulses first perceptible in the third quarter of the twelfth century, it persisted in upper Occitania well into the reign of Louis IX, it faded more rapidly in lower Languedoc, where royal government was instituted as early as 1226, and it ceased to be legislated in southern France after 1226 (Provence) and 1233 (Toulouse and Quercy).<sup>110</sup>

The organized peace can perhaps be regarded as the final expression of a basically Carolingian form of government. For all its pragmatic originality, it preserved the old condominia of higher clergy and baronage which, as we are now learning, had survived the age of ecclesiastical reform.<sup>111</sup> It flourished only where older notions of fidelity and property had survived or where lay powers had failed the Carolingian tests. It was adaptable to regimes feudal, patrimonial, allodial, or urban, which it purported to secure, not replace. The southern military aristocracies might grumble about its restrictions, as did the Catalan barons under Peter II, but they had no thought of overturning it, for they had never imagined a feudalism either productive or destructive of public order.

<sup>109</sup> I attempted to make this clear in *Assemblies in Languedoc*, 112, 115, 118–21, 124, 233; for the *sénéchaussées* of lower Languedoc, see *HL*, 8: 1422–26, 1705–06. Royal authority had penetrated too early and too often in Berry and Auvergne for the organized peace to develop the autonomous strength it had in some southern dioceses of the province of Bourges; that the kings understood the geopolitical circumstances is made perfectly clear by the “golden bull” for Gévaudan, 1161 (*LTC*, 1: no. 168): “Tota terra illa [Gévaudan] difficilima aditu et montuosa, in potestate episcoporum semper extiterit, non tantum ad faciendam ecclesiasticam censuram, sed etiam ad iudicandum in gladio super illos quos culpa sua monstrabat sic redarguendos.”

<sup>110</sup> The statutes of Toulouse, chiefly concerned with heresy, merely allude to the peace as still in force, *HL*, 8: 963–69; for the new temporary confederation in Quercy, see above, note 88.

<sup>111</sup> See Magnou-Nortier, *Société laïque et l'église*, chs. 3, 4, 6, and 550–64; cf. Bonnassie, *Catalogne*, 1: ch. 2. As Bonnassie shows, Catalan public structures were in most respects less influenced by Frankish than by Visigothic law, but the higher ecclesiastical order was subject to Narbonne in the generations when the (Carolingian) peace movement spread to Catalonia. Breaches of the peace were to be prosecuted “utroque gladio,” according to the council fathers in 1215, Mansi, 22: 948, art. 39.

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## Spanish Wool and Dutch Rebels: The Middelburg Incident of 1574

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and  
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BECAUSE OF THEIR ABSORPTION in a narrowly defined Renaissance-Reformation, historians of early modern Europe long tended to focus on Italy, Germany, and England, to the neglect of France and particularly Spain. Partly for the same reason, the period 1400–1550 once received far more attention than did the next century and a half. More recently a growing interest in economic and social development, the consequence of contemporary problems of modernization and economic growth, has stimulated research in the period 1550–1700. No longer can that period be passed over as an unimportant transition between the Renaissance-Reformation and the Enlightenment, and no longer can Spain be safely ignored, since the early modern period spans the time of its greatest power and of its decline. Indeed, two major scholarly concerns—“the price revolution of the sixteenth century” and “the crisis of the seventeenth century”—fit the Spanish experience better than that of other areas.<sup>1</sup> As historians unravel the tangled story of Spain’s fall from power, they are taking a fresh look at the revolt of the Netherlands, particularly in its economic implications. For eighty years (1568–1648) the revolt drained tax revenue from Castile and its American empire and weakened the fragile links

We would like to thank the Office of International Programs and the Graduate School of the University of Minnesota and the San Diego State University Foundation for grants which financed our research. We also owe a debt of affection and gratitude to Selma Barkham, who first directed our attention to this case and shared her expert knowledge of northern Spanish archives with us. James Tracy of the University of Minnesota read an earlier version of this article and made several helpful suggestions.

<sup>1</sup> For sixteenth-century growth in Castile see Earl J. Hamilton, *American Treasure and the Price Revolution in Spain, 1501–1650* (Cambridge, Mass., 1934; reprinted. New York, 1965); José Gentil da Silva, *En Espagne: Développement, subsistance, déclin* (Paris, 1965); and Ladislav Reitzer, “Some Observations on Castilian Commerce and Finance in the Sixteenth Century,” *Journal of Modern History*, 32 (1960): 213–23. Commentators on Hamilton’s thesis are legion and should be approached through the bibliographies of individual countries. For Spain, a useful critique is Jorge Nadal, “La revolución de los precios españoles en el siglo XVI: Estado actual de la cuestión,” *Hispania, revista española de la historia*, 19 (1959): 503–39. The best starting point for the seventeenth-century crisis is Trevor Aston, ed., *Crisis in Europe, 1560–1660* (London, 1965), which reprints the major contributions to the debate up to 1965. See also Pierre Vilar’s brilliant “Le temps du ‘Quichotte’,” *Europe*, 34 (1956): 3–16, now available in English in Peter Earle, ed., *Essays in European Economic History 1500–1800* (Oxford, 1974).

connecting the Habsburg possessions in Europe.<sup>2</sup> The revolt also disrupted Castile's lucrative wool trade with Flanders, a trade that had been synonymous with Castilian prosperity since long before the discovery of the New World.

One incident during the revolt illustrates in extraordinary detail the internal workings of the wool trade and the pressures to which it was subject in wartime. When Dutch rebels took the garrison town of Middelburg in February 1574, they also captured a large store of warehoused wool, originally consigned to the Spanish wool staple in Bruges.<sup>3</sup> The insurers in Burgos refused to pay off on the losses, and the shippers filed suit for their claims, first in the *Consulado* of Burgos and then in the royal court of appeals in Valladolid (the Chancellery).<sup>4</sup> The case dragged on for five years, leaving behind a voluminous record that allows us to reconstruct the Middelburg incident and its aftermath. The fate of this one shipment occupied those involved from 1570 to 1580, a decade which was, incidentally, crucial for the internal economy of Castile.<sup>5</sup> The capture of Middelburg and its store of wool was but one episode in Spain's long decline, but not an unimportant one. It dealt a serious blow to business confidence in Old Castile and probably the decisive blow to the maritime insurance industry of Burgos.

In the economic boom of late fifteenth- and sixteenth-century Europe, Spain was an early leader. Its population expanded after about 1450, and the commercial and financial fairs in Medina del Campo became famous throughout Europe.<sup>6</sup> In part, this was because Spanish products such as fruit, sugar, saffron, and (above all) fine wool benefited from the international market for luxury goods after the Black Death.<sup>7</sup> The Spanish merino sheep, a cross between native and North African sheep,<sup>8</sup> produced wool that became the standard for quality in the fifteenth century, and indeed remains so today.<sup>9</sup> The extensive land area of Castile and León, much of it unsuitable for

<sup>2</sup> See Geoffrey Parker, *The Army of Flanders and the Spanish Road, 1567–1659* (Cambridge, 1972), and his more recent "Why Did the Dutch Revolt Last Eighty Years?" *Transactions of the Royal Historical Society*, 26 (1976): 53–72. For taxation in Castile, see Modesto Ulloa, *La hacienda real de Castilla en el reinado de Felipe II* (Rome, 1963), and Antonio Domínguez Ortiz, *Política y hacienda de Felipe IV* (Madrid, 1960).

<sup>3</sup> The rebels sold the captured wool to Flemings established in England and to merchants in La Rochelle, Orleans, and Rouen. In addition, they tried to entice Flemish and English drapers to establish a wool staple in Holland, evidently to undercut the drapery industry of Flanders. Valentín Vázquez de Prada, *Lettres marchandes d'Anvers* (4 vols., Paris, 1960), 1:29.

<sup>4</sup> Manuel Basas Fernández, *El Consulado de Burgos en el siglo XVI* (Madrid, 1963) is a general survey of the powerful merchant association that dominated maritime insurance and the wool trade in Old Castile. We have used the Spanish word *Consulado* in this article, since the English equivalent is misleading. Chancellery, however, is an adequate translation of *Chancillería*.

<sup>5</sup> Harvest failures were reported in many parts of Castile in the late 1570s, particularly in 1579, the year many historians see as the turning point in Castilian agriculture. See Felipe Ruiz Martín, "Los hombres de negocios genoveses de España durante el siglo XVI," *Fremde Kaufleute auf der Iberischen Halbinsel*, ed. Hermann Kellenbenz (Cologne, 1970), 97–98.

<sup>6</sup> Jorge Nadal, *La población española: Siglos XVI a XX* (Barcelona, 1966); Cristóbal Espejo and Julián Paz, *Las antiguas ferias de Medina del Campo* (Valladolid, 1909).

<sup>7</sup> Harry A. Miskimin, *The Economy of Early Renaissance Europe, 1300–1460* (Englewood Cliffs, N.J., 1969), 61–65.

<sup>8</sup> Robert S. López, "El origen de la oveja merina," *Estudios de historia moderna*, 4 (1954).

<sup>9</sup> Jaime Vicens Vives, with Jorge Nadal Oller, *An Economic History of Spain*, tr. Frances M. López-Morillas (Princeton, 1969), 357; Frédéric Mauro, *Le XVI<sup>e</sup> siècle européen: Aspects économiques* (Paris, 1966), 145. For the characteristics of merino wool, see K. G. Ponting, *The Wool Trade: Past and Present* (Manchester and London, 1961), 33–34.

farming, provided year-round pasture for vast transhumant flocks of sheep. More important, the early political consolidation of Castile and León and the crown's need for money fostered royal protection of sheepherding interests with the famous Mesta, an association primarily for owners of transhumant sheep.<sup>10</sup> Some merino wool was woven by local industry, but from the thirteenth century fine wool became the staple of Spain's export trade, particularly to the textile centers of the Netherlands. Long a rival of English wool, Castilian merino wool had become by the second half of the fifteenth century the preferred raw material of the fine drapery industry of Flanders.

In many ways the wool trade was the keystone of the Castilian economy and influenced Spain's relations with the rest of Europe. Spain's growing prosperity and heightened power led to diplomatic marriages between the Habsburgs and the children of Ferdinand and Isabella and unexpectedly to the huge inheritance of their grandson Charles V. As sovereign of both Castile and the Netherlands, Charles did much to strengthen trading links between the two areas, and, despite disturbances elsewhere in his monarchy, the wool trade continued to flourish. Early in his reign there were over three million migratory sheep and about one and a half million nonmigratory ones under Mesta jurisdiction.<sup>11</sup>

During the reign of Philip II, many European countries turned against Spain, either because of its political power or because it was the foremost defender of Catholicism in the era of the Protestant Reformation. One inexpensive way to undermine Spanish power was to encourage privateers to prey on her commercial vessels, particularly along the vulnerable route from the north coast of Spain to the Netherlands. In 1560 England established ties with the French Huguenots and sent troops to their coastal towns of Le Havre, Dieppe, and Rouen. Shortly thereafter, both Huguenot and English privateers began sailing under letters of marque from Huguenot authorities and from Elizabeth of England. Though their enemy was supposedly France, their targets were more often Spanish commercial vessels.<sup>12</sup> When the Netherlands revolt broke out, England allowed rebel ships the clandestine use of her ports. In 1568 relations between England and Spain became openly hostile for the first time when five Spanish vessels bound for Antwerp were seized on taking refuge in English ports to avoid Huguenot privateers. Elizabeth impounded the cargoes, including 155 chests containing money destined for the Duke of Alba's troops in the Netherlands. In response, Alba impounded English possessions in the Low Countries, and, although restitution was negotiated in 1574, the English continued for five years to impound Spanish goods and ships.<sup>13</sup> By 1571 they had sequestered twelve more vessels from the Cantabrian coast between Santander and the French border (about five percent of all vessels registered in that important region). Another ten ships were taken by French

<sup>10</sup> Julius Klein, *The Mesta: A Study in Spanish Economic History* (Cambridge, Mass., 1920).

<sup>11</sup> Jean-Paul Le Flem, "Las cuentas de la Mesta, 1510-1709," *Moneda y Crédito* 121 (1972): 38.

<sup>12</sup> Richard Bruce Wernham, *Before the Armada. The Emergence of the English Nation, 1485-1588* (New York, 1966), 281-82.

<sup>13</sup> Conyars Read, "Queen Elizabeth's Seizure of the Duke of Alba's Pay Ships," *Journal of Modern History*, 5 (1933): 443-64; Wernham, *Before the Armada*, 296-97.



Protestants.<sup>14</sup> It is not surprising that Philip II came to favor a convoy system to protect shipping between northern Spain and the Flemish ports and asked Alba to provide such a plan in 1569.<sup>15</sup>

From 1568 Philip prohibited commercial sailing to Flanders, except by special license. In July 1570 he gave the *Consulado* of Burgos permission to name one or two captains to command a fleet of wool ships to Flanders.<sup>16</sup> Antonio de Bertendona, a citizen of Bilbao and owner-captain of the 670-ton *nao Nuestra Señora de Begoña*, was their first choice—an ill-fated one, as it turned out. The *Begoña* was loaded with wool, and the fleet was ready to sail in September 1570.<sup>17</sup> Bertendona seems to have made one attempt to leave port, but he quickly turned back, perhaps because of the bad weather so common that late in the season.<sup>18</sup> By the first months of 1571 Bertendona was dead, and the *Begoña*, under the control of his heirs, was still loaded and ready to sail from Santander.<sup>19</sup> But Philip did not authorize the *Consulado* to name a new fleet captain until July of that year.<sup>20</sup> The long delays bred impatience among some of the coastal captains. Juan de Enderica left port in his 450-ton *La Concepción*, loaded with wool for Flanders; he may not even have had a license. With him, or perhaps sailing separately, was Ochoa de Larrea and his 600-ton wool ship *Nuestra Señora de Portugalete*. On June 8 a royal agent Cristóbal Barro reported the unwelcome news that both ships had been captured off the French coast by privateers of the Prince of Orange.<sup>21</sup>

To avoid similar disasters and further delay, Philip decided to send with the remaining wool ships a military detachment under the newly designated governor of the Netherlands Juan de la Cerda, Duke of Medinaceli.<sup>22</sup> In charge of victualing the fleet and providing space for the soldiers who would accompany it was the Vizcayan Juan Martínez de Recalde, who later figured prominently in the Great Armada of 1588.<sup>23</sup> When Medinaceli arrived on the north coast, he worked diligently to hasten preparations. At the end of

<sup>14</sup> Archivo General de Simancas (hereafter AGS), *Guerra antigua*, leg. 75, folio 13. The section is sometimes cited as *Guerra y marina*.

<sup>15</sup> Alba to Philip II, Brussels, 25 September 1569, *Colección de documentos inéditos para la historia de España* (hereafter *Codoin*), ed. Martín Fernández Navarrete et al., (112 vols.; Madrid, 1842–95), 38: 197–98.

<sup>16</sup> AGS, *Guerra antigua*, leg. 74, fols. 28, 32; Basas, *Consulado*, 181.

<sup>17</sup> Archivo de la Real Chancillería de Valladolid (hereafter ARCV), *Sección de Pleitos Civiles, Masas* (hereafter *Masas*), leg. 260, *caja* 1045–1. More wool was loaded thereafter. Much of the documentation for this study comes from ARCV, *Masas*, legs. 260–61. The two *legajos* are composed of some ten boxes, each about eight inches deep. The pages are not usually foliated, but when folio numbers appear, they will be cited.

<sup>18</sup> Basas, *Consulado*, 181.

<sup>19</sup> AGS, *Guerra antigua*, leg. 75, fol. 13. The *Begoña* delayed sailing, according to Juan Martínez de Recalde, because of debts, the lack of a master, and a general lack of confidence in the vessel on the part of the wool owners; *ibid.*, leg. 75, fol. 55.

<sup>20</sup> *Ibid.*, leg. 74, fol. 32.

<sup>21</sup> *Ibid.*, fols. 5–6, 48–49.

<sup>22</sup> ARCV, *Masas*, leg. 261, *caja* 1048–1; Basas, *Consulado*, 181. Philip II first ordered Medinaceli to replace the Duke of Alba, then merely to succeed him. In any case, Alba declined to step down until November 1573, when, broken in health, he was quite willing to be replaced by Luis de Requesens. See *Codoin*, 35–36 for the royal correspondence on this and related matters. Also helpful is A. W. Lovett, “A New Governor for the Netherlands: The Appointment of Don Luis de Requesens, Comendador Mayor de Castilla,” *European Studies Review*, 1 (1971): 89–103; and the same author’s “The Governorship of Don Luis de Requesens, 1573–1576. A Spanish View,” *European Studies Review* 2 (1972): 187–99.

<sup>23</sup> AGS, *Guerra antigua*, leg. 74, fols. 5–6, 48–49.

September 1571 Philip wrote him that the royal contingent should consist of five *naves* and two *zabras* leased from local masters.<sup>24</sup> Later three small ships were added to the royal contingent, but the vast majority of the more-than-forty ships were privately operated.<sup>25</sup> Even with nine of the ships carrying artillery pieces and with 1,263 soldiers aboard, the "armada" of Medinaceli remained largely a commercial fleet in purpose and composition. Though royal documents imply that private vessels carried all the wool, at least three of the chartered royal ships also had woolsacks in their holds.<sup>26</sup>

Following normal procedures, the wool we are concerned with had been brought from its points of origin through Burgos and other cities to be washed, graded, and sacked. After that was done, pack animals would carry it to the north coast for freighting by commercial agents. In this case, all of the wool was insured by 147 members of the Burgos *Consulado*, fifty-nine of whom were citizens of Burgos, while eighty-eight were from Medina del Campo, Valladolid, and other places.<sup>27</sup> Each of the insurance policies later filed with the Chancellery covered a given shipper and his wool, and the risk was shared among a number of insurers. The premiums cost each shipper between nine and thirteen percent of the value of his wool, depending on the going rate when he purchased the policy and the reputation of the ships and captains available for freighting. A *Consulado* agent on the coast had the right to make the final selection of ships and to oversee the loading. The policies left the precise route of the voyage open, because of the vagaries of the sea and the weather. They generally mentioned the usual lightering point of Arnemuiden (called Ramua by the Spanish), but it was quite clear that the wool staple of Bruges was the final destination.

The insurance policies and other supporting documents provide more detailed information on the wool itself and on the men who shipped it. In the absence of serial data on the wool trade (which we hope to develop in the future), these documents are particularly valuable. Of the eighty-two shippers involved in the Middelburg case, Miguel de Salamanca of Burgos had the

<sup>24</sup> Philip II to Medinaceli, Madrid, 30 September 1571 (misprinted as 1581), *Codoin*, 35: 435–36.

<sup>25</sup> Cesáreo Fernández Duro, *La armada española* (9 vols.; Madrid, 1895–1903, 2:266–68; *Codoin*, 36: 5–8. Sixteenth-century Spaniards grouped vessels in loose categories according to size. *Naos* and *naves* could range from 60 to 800 tons, though most here were from 200 to 600 tons. *Navíos* were smaller, generally from 20 to 120 tons. *Zabras*, the smallest type of ship with Medinaceli's armada, ranged from 20 to 70 tons. Galeasses and galleons were primarily war vessels, but several from 35 to 120 tons were used here commercially. AGS, *Guerra antigua*, leg. 75, fol. 13. A recent summary of work on Spanish shipping is Lawrence A. Clayton, "Ships and Empire: The Case of Spain," *The Mariner's Mirror*, 62 (August 1976): 235–48.

<sup>26</sup> ARCV, *Masas*, leg. 261, *caja* 1047–1. Fernand Braudel is mistaken in suggesting that the fleet was assembled primarily to carry money to Flanders. *The Mediterranean and the Mediterranean World in the Age of Philip II*, tr. Siân Reynolds (2 vols.; New York, 1972–73), 1:483. In fact, no study we have found has a clear idea of the composition and the purposes of the fleet, which were obscured by the imperfect distinction between commercial and military vessels. "[I]n the sixteenth century there were no warships which did not transport merchandise, and the dividing line between merchant ships and armada ships is extremely vague." Pierre and Huguette Chaunu, "The Atlantic Economy and the World Economy," in Earle, ed., *Essays in European Economic History*, p. 115. This is a translation of a very useful article in *Cahiers d'histoire mondiale* (1953) that summarized their multivolume work *Seville et l'Atlantique (1504–1650)* (8 vols., Paris, from 1955).

<sup>27</sup> Lists of shippers and insurers appear in various places in ARCV, *Masas*, legs. 260–61.

largest claim against the insurers—12,275 ducats on 525 sacks of wool. Martín Martínez from the village of Torrecilla en Cameros in Logroño had the smallest claim—150 ducats on four sacks.<sup>28</sup>

Table 1 shows the geographical spread of all the shippers, grouped by modern provinces for clarity. Their residences ranged from Segovia north to Vizcaya, and from Valladolid east to Navarra. This region contained summer pastures for the transhumant flocks of the Mesta after they were sheared and their wool prepared for export. All the towns mentioned, except Escároz and Bilbao, were located on or near the important Segovian *cañada* (sheep path) of the Mesta.<sup>29</sup> Escároz is in Navarra adjacent to Logroño, and Bilbao was and is a major port, but both towns together contributed only three shippers and less than one percent of the total amount of wool. Forty percent of the shippers came from the city and province of Segovia. So great was their participation that the documents often describe the case as being between the shippers of Segovia and the insurers of Burgos, as if no one else had been involved. But other places were also represented on both sides. For example, Burgos, the center of the insurers, was also the nearest rival to Segovia in shippers, with twenty individuals sending 1,883 sacks. It was farther from the main shearing places, but its strong merchant community usually supplemented local production with wool purchased elsewhere.

The average value of a sack of wool is an interesting, though problematical, figure. There were recognized standards of wool quality, and the job of grader was a highly skilled and well-paid occupation. Unfortunately, the documents only rarely state more than a simple total for all the sacks sent by a given shipper. In the few detailed lists there was often a wide range in value even for wool from the same region and the same shipper. The wool sent by Alonso Díez de Modoya and Juan Díez de Loyola of Carrión ranged in value from 35.5 ducats per sack for first-quality wool (with nettles in it) from transhumant sheep to 16.1 ducats per sack for second-quality wool from stationary sheep. First-quality clean wool (*florete*) was worth about 42.7 ducats per sack, including the costs of preparation, insurance, transportation to the coast, and freighting.<sup>30</sup> By the time a sack of *florete* safely reached its destination, it could fetch three times that figure on the market.<sup>31</sup>

Unfortunately, Table 1 does not allow us to map the quality of Castilian wool. We have figures only for wool lost in Middelburg, and there is no way to know if that was a representative sample of production. More important, we cannot assume that the origin of a shipper was necessarily the origin of the wool he shipped. Leading merchants engrossed wool production from all over

<sup>28</sup> Only seventy shippers filed claims. The rest either had loads too small to justify litigation, or they were shipper-insurers who would lose more if the crown decided against the insurers. ARCV, *Masas*, leg. 260, *caja* 1045-1; leg. 261, *caja* 1047-1.

<sup>29</sup> Klein, *Mesta*, 19 and map between pages 18 and 19. The map is reproduced in Vicens, *Economic History of Spain*, 254, and in Braudel, *Mediterranean*, 1:93.

<sup>30</sup> ARCV, *Masas*, leg. 261, *caja* 1048-1.

<sup>31</sup> Woolsacks bound for Flanders each weighed 8.5 *arrobas*, or about 212.5 pounds. Those bound for Italy weighed 10 *arrobas*. Basas, *Consulado*, 253, 261.

TABLE 1.  
SHIPPERS, WOOL, AND PRICE BY PROVINCES

PROVINCE Towns	<i>Persons</i>	<i>Total Sacks</i>	<i>Sacks per Person</i>	<i>Total Ducats</i>	<i>Ducats per Sack</i>
<b>BURGOS</b>					
Belorado	2	24	12	700	29.2
Burgos	16	1,579	98.7	32,160	20.4
Castrojeriz	2	280	140	6,291	22.5
Provincial Totals	20	1,883	94.15	39,151	20.79
<b>LOGROÑO</b>					
Logroño	3	100	33.33	2,905	29.05
Nájera	3	242	80.67	5,693	23.5
Torrecilla en Cameros	1	4	4	150	37.5
Provincial Totals	7	346	49.43	8,748	25.28
<b>NAVARRA</b>					
Escároz	1	24	24	600	25
Escároz	1		(missing data)		
Provincial Totals	1   (1)	24	24	600	25
<b>PALENCIA</b>					
Carrión	4	347	86.8	6,435	18.5
Palencia	1	116	116	3,350	28.9
Provincial Totals	5	463	92.6	9,785	21.13
<b>SEGOVIA</b>					
Becerril	1	357	357	6,716	18.8
Segovia	26	2,815	108.3	67,327	23.9
Segovia	8		(missing data)		
Sepúlveda	1	10	10	300	30
Villacastín	1		(missing data)		
Provincial Totals	28 (9)	3,182	113.64	74,343	23.36
<b>SORIA</b>					
Almazán	1	9	9	230	25.6
Soria	2	33	16.5	504	15.3
Soria	2		(missing data)		
Provincial Totals	3   (2)	42	14	734	17.48
<b>VALLADOLID</b>					
Medina del Campo	3	57	19	1,360	23.9
Medina de Río seco	1	115	115	1,500	13
Valladolid	1	22	22	550	25
Provincial Totals	5	194	38.8	3,410	17.58
<b>VIZCAYA</b>					
Bilbao	1	26	26	600	23.08
Provincial Totals	1	26	26	600	23.08
<b>GRAND TOTALS</b>	70 (12)	6,160	88	137,371	22.3

Spain, sending their agents out with account books (*libros de la sierra*) long before the shearing.<sup>32</sup> The documentation is also silent on the progress of raw wool from shearing to embarkation points. Provincial notarial archives should be helpful in clarifying this, but they have not yet been studied. One thing is certain. By no means did all wool passing north have to go through Burgos. As important as that city was, the wool trade was large enough to involve towns and villages along several alternate routes to the coast,<sup>33</sup> not just the Burgos to Santander (or Bilbao) axis we often read about.

The shipping business was also spread among several ports in normal years, although Santander was clearly pre-eminent in the early sixteenth century. Since Medinaceli's armada had to be coordinated for efficiency, most of its wool was loaded in Santander. Even so, at least three large ships were loaded in Deva near San Sebastián,<sup>34</sup> and the ships themselves were registered in ports all along the Cantabrian coast.

Ten ships figured in the Middelburg claims, among them the "capitana" *Crucifijo de Burgos*, owned by the new captain of the armada Juan de Montellano, a citizen of Portugalete. Bad luck had continued to bedevil the original "capitana" *Nuestra Señora de Begoña* after the death of its owner Antonio de Bertendona. Sometime before March 1572 it had run aground on a sandbar off Laredo ("se perdió en el arenal de Laredo"), and 1,175 of the wool-sacks it carried were transferred to other vessels.<sup>35</sup> The *Begoña* itself never made the trip. Montellano's *Crucifijo*, although the same size as the *Begoña* (600–750 tons), carried only about 135 sacks of wool. Medinaceli traveled in the "almiranta," Ochoa de la Sierra's 470-ton *La Asención*, registered in Portugalete. Jacobo de Jáuregui loaded his *nao Nuestra Señora de Begoña* (a different ship from Bertendona's) in Deva and joined the armada in Santander. Juan de Navejas commanded *La Trinidad*, which he owned with his mother María, nicknamed "la marquesa de Villaviciosa." The other ships were Martín de Capitillo's 540-ton *Nuestra Señora de la Concepción*, registered in Portugalete; San Pedro de Arbieto's 550-ton galleon (or *nao*) *Nuestra Señora de Aránzazu*, registered in Bilbao; Juan de Ebora's *San Juan* from Pasajes; Juan de Regoitia's *Santo Crucifijo* from Bilbao; Pedro de Ebora's *San Nicolás*; and Juan de Vallecillo's *San Felipe y Santiago*.<sup>36</sup> Some of the wool they carried had been sheared as early as 1568.

These ten ships and over thirty others eventually left for Flanders with Medinaceli, but not before more maddening delays. Medinaceli wrote Philip on December 15, 1571, telling him the fleet was ready to depart,<sup>37</sup> but four days later bad weather forced it back into port. For safety, Medinaceli

<sup>32</sup> *Ibid.*, 235–38.

<sup>33</sup> The best recent work on internal Castilian trade routes, although for a slightly earlier period, is Jean-Pierre Molénat, "Chemins et ponts du nord de la Castille au temps des Rois Catholiques," *Mélanges de la Casa de Velázquez* (1971), 115–62, and maps.

<sup>34</sup> AGS, *Guerra antigua*, leg. 75, fol. 20.

<sup>35</sup> ARCV, *Masas*, leg. 261, caja 1047–1.

<sup>36</sup> ARCV, *Masas*, leg. 260, caja 1041–4; AGS, *Guerra antigua*, leg. 75, fol. 13.

<sup>37</sup> Medinaceli to Philip II, Laredo, 15 December 1571, *Codoín*, 35: 484–85.

unloaded the silver.<sup>38</sup> By the twenty-fourth he had suspended sailing indefinitely and disembarked his troops.<sup>39</sup>

All through the next spring Medinaceli and the king exchanged letters, Philip urging a speedy departure, Medinaceli reporting numerous difficulties. Bad weather stalled new sailing preparations in mid-April 1572.<sup>40</sup> When the ships finally left Laredo on the last night of the month,<sup>41</sup> strong winds from the northwest reduced their chances to enter the English Channel and began to blow them toward the French coast. Medinaceli and his captains decided to return to Spain, fighting a wind that scattered the fleet and blew units of it into Santander, Laredo, and Castro Urdiales. Shortly thereafter, the hazardous conditions of the north coast claimed another commercial vessel, the sixty-ton *Santiago* of Pedro de Bayona. Named in several insurance policies for the Flanders voyage, it was declared unseaworthy and remained behind in Laredo.<sup>42</sup>

Anxious to avoid further delays, Medinaceli quickly reassembled his fleet and reprovisioned it. As a precautionary measure he held the soldiers under guard in Santander to prevent desertions.<sup>43</sup> The resupply seems to have gone well. At the last minute a problem arose between the masters of the wool ships and the *Consulado's* agents on the coast, and for a time Medinaceli feared that the wool ships might not sail at all.<sup>44</sup> Nonetheless, they all put out to sea on May 14th. Having already waited so long, they should have waited a few days more. A letter from the king arrived too late to warn them that Flushing (called Vlissingen locally and Frexelingas by the Spanish) had revolted, endangering their passage through the Scheldt.<sup>45</sup> Unaware of this new development, the fleet sailed up the French coast. The passage was uneventful, except for the loss of the 630-ton *San Salvador* of Ochoa and Pedro de Capitillo, which hit rocks and sank with four or five men, a number of horses, several artillery pieces, and 140 sacks of wool.<sup>46</sup> As the remainder of the fleet made its way through the English Channel, Medinaceli finally learned from some fishermen that Flushing and perhaps all of Walcheren Island were in rebel hands.

This could mean only trouble for the Spanish. Walcheren Island in Zeeland contained several towns of critical importance to the Flanders trade. Flushing

<sup>38</sup> Medinaceli to Philip II, Laredo, 19 December 1571, *Codoin*, 35: 485–87.

<sup>39</sup> Medinaceli to Philip II, Laredo, 24 December 1571, *Codoin*, 35:487–92.

<sup>40</sup> Medinaceli to Philip II, Laredo, 15 April 1572, *Codoin*, 35:558–60; Medinaceli to Philip II, at sea off Laredo, 27 and 30 April 1572, *Codoin*, 35:570–74.

<sup>41</sup> Juan Martínez de Recalde to Zayas, Laredo, 1 May 1572, *Codoin*, 36:11–12; Medinaceli to Philip II, Santoña, 1 May 1572, *Codoin*, 36: 12–13.

<sup>42</sup> ARCV, *Masas*, legs. 260, 261. It appears on the list from AGS, *Estado*, leg. 552, reprinted in *Codoin*, 36: 5–8, and Fernández Duro, *Armada española*, 2: 266–68.

<sup>43</sup> Medinaceli to Philip II, Santander, 7 May 1572, *Codoin*, 36: 13–18. Geoffrey Parker, *The Army of Flanders and the Spanish Road*, 30–33, reports numerous contemporary statements that the discipline of troops increased in direct proportion to the distance from their homes.

<sup>44</sup> Medinaceli to Philip II, Santander, 13 May 1572, *Codoin*, 36: 19–20.

<sup>45</sup> Philip II to Medinaceli, Madrid, 15 May 1572, *Codoin*, 36: 24–25.

<sup>46</sup> Medinaceli to Philip II, Brussels, 24 June 1572, *Codoin*, 36: 78–82. The Capitillos' *San Salvador* is the only vessel that fits the description given by Medinaceli, although he did not mention it by name.



on the southern coast controlled the entrance to the Scheldt.<sup>47</sup> Middelburg in the island's center was a well-fortified commercial and garrison town. It was linked to the straits separating Walcheren from the rest of Zeeland by a canal to Arnemuiden on the island's east coast. The latter town was the habitual lightering point for Bruges, and it was protected by the castle of Rammekens east of Flushing. The strategic importance of the island, and particularly of Flushing, had been pointed out to Philip II as early as 1564 by his half-sister Margaret of Parma, who was also his first governor in the Netherlands.<sup>48</sup> Unfortunately, Philip and Alba ignored Flushing until it was too late.

Queen Elizabeth had finally expelled Dutch freebooters from their English havens in March 1572. Ironically, this caused the Spaniards even worse trouble. On April 1, the rebel captain William de la Marck took Brill in Holland without a fight, thus establishing a rebel strongpoint near Zeeland. When Alba sent more Spanish troops to Flushing, the citizens not only refused them entry but expelled the garrison already there, with aid from Brill and from England. Spanish forces were able, however, to retain Middelburg and to reconquer Arnemuiden after a brief struggle.<sup>49</sup>

Medinaceli's fleet anchored off the Flemish coast at Blankenberg on the morning of June 10. His primary responsibility, of course, was not the wool ships, but the soldiers and money destined for the Duke of Alba. Most of these he unloaded at Sluys (called la Esclusa by the Spanish), even though several ships went aground on the sandbanks of the Zwin. From Sluys Medinaceli proceeded to Brussels,<sup>50</sup> leaving the commercial fleet under the command of Juan Martínez de Recalde.<sup>51</sup> At least four small wool ships probably unloaded safely at Sluys: Hernando de Somado's sixty-ton *Santa Clara*; Juan de Somado's sixty-ton *San Juan*; Domingo de Villaviciosa's 70- to 120-ton *Santa María*; and Juan de Ximeno's 110- to 120-ton *Nuestra Señora la Blanca*.<sup>52</sup> The rest of the wool ships, including eight too large even to attempt the Zwin, remained on the Flemish coast.

They could not remain there long. Worsening weather forced them to make for Arnemuiden, and, as they moved away from the coast, a fleet of twenty-three Flemish ships bound for Antwerp out of Lisbon passed them at a good clip. Since the Flemish captains had not heard of the rising of Flushing, they anchored peacefully in the harbor and were quickly seized and boarded by the

<sup>47</sup> An anonymous contemporary account that gives detailed sailing directions from the Cantabrian coast to Flanders calls for pilots to be picked up in Flushing for the passage through the Scheldt. *Codoin*, 75: 31–32. Even today, the same is true. *Ports of the World*, 1976 (London, 1976), 140.

<sup>48</sup> Jervis Wegg, *The Decline of Antwerp under Philip II of Spain* (London, 1924), 149–50. Even the English mercenary Roger Williams opined that Alba should have completed the citadel of Flushing before that of Antwerp. Williams, *The Actions of the Low Countries*, ed. D. W. Davies (Ithaca, N.Y., 1964), 34–36.

<sup>49</sup> Bernardino de Mendoza, *Comentarios de lo sucedido en los Países-Bajos desde el año de 1567 hasta el de 1577* [first published, Madrid, 1592], *Biblioteca de autores españoles*, vol. 28 (Madrid, 1853), 447–50, writing a decade after the event, gave the Spanish commander's name as Osorio de Angulo. An anonymous Spanish account dated 23 May 1572 called him Beauvois. *Codoin*, 31: 24. On the English intervention, see Williams, *Actions of the Low Countries*, 54–60.

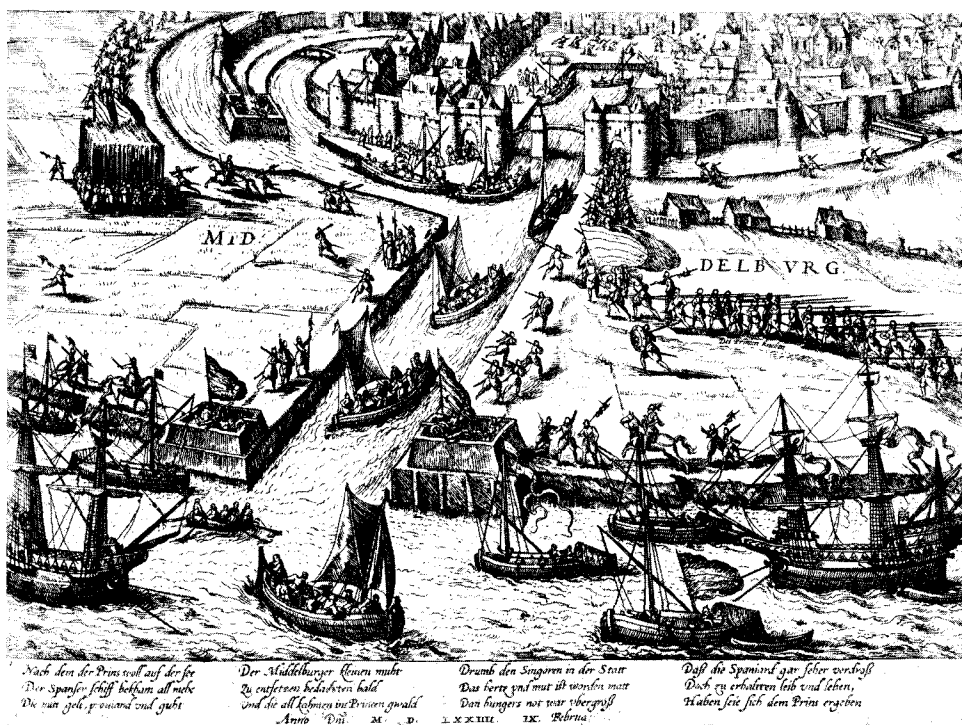
<sup>50</sup> Medinaceli to Philip II, Sluys, 11 June 1572, *Codoin*, 36: 25–26; Mendoza, *Comentarios*, 454.

<sup>51</sup> Medinaceli to Alba, Sluys and other places, 10–17 June 1572, *Codoin*, 36:29–51, particularly p. 38. Mendoza, *Comentarios*, 454, mistakenly gives the name Juan Osorio de Recalde.

<sup>52</sup> ARCV, *Masas*, legs. 260–61.



Walcheren and straits. In Matthijs Smallegange, *Nieuwe cronyk van Zeeland*. Middelburg: Joannes Meertens. 1696. Photo courtesy of the James Ford Bell Library, University of Minnesota.



The fall of Middelburg, 9 February 1574. In *Collection of Engravings Illustrating Historical Events from 1550–1610*. [Cologne?, 1615?] Photo courtesy of the Trustees of the British Museum.

rebels. When the Spaniards passed, the Flushingers were able to shoot at them with guns on the Flemish ships as well as with shore-mounted guns!<sup>53</sup> The bombardment itself seems to have done little damage, but for one reason or another the fleet lost three more ships before reaching Arnemuiden on June 13th. Ochoa de la Sierra's *La Asención*, the "almiranta," lost its anchor and went aground on a sandbar, although its crew and cargo were saved.<sup>54</sup> Claes Bausen (or Bauen) and his *León Rojo* were either captured by the Flushing rebels or joined them voluntarily. This was the only Flemish vessel involved in Medinaceli's armada, and it had been impressed into service in Santander. In addition, the *San Nicolás* of Pedro de Eborá was lost somewhere in Zeeland, although it is unclear just when and how the loss occurred.<sup>55</sup>

With the small fleet anchored off Arnemuiden, two Spanish residents of Middelburg, Gregorio de Ballo and Pedro de Arnedo, arrived with lighters to ferry the wool to Bruges. The armada's commanders ordered them to deliver the wool instead to Middelburg for safekeeping. This greatly displeased Ballo and Arnedo, who did their work reluctantly and allowed the wool to be dumped in the streets. The Castilian consuls in Bruges sent Juan Castro de la Loo to receive the wool and try to move it to Bruges. Failing that, he was to warehouse it in Middelburg. When Castro arrived, the captains had already unloaded half the wool and refused him permission to move it. Castro had no choice but to gather it from the streets, repair broken sacks damaged by the soldiers, and find suitable warehouses. Once all the wool was unloaded, the captains obtained receipts from Castro, left their ships in Middelburg, and went to Bruges to be paid for the voyage. Returning, they paid their crews. Castro and other agents who joined him from Bruges continued to seek permission to move the wool, but the responsible authorities would not grant it. As a result, the wool remained in Middelburg until the whole town fell to the rebels on February 18, 1574.<sup>56</sup>

It is important to establish the events leading to the capture of Middelburg and its warehoused wool, because the subsequent disputed insurance claims hinged on two key issues: 1.) under what circumstances the wool was taken to Middelburg in the first place and why it remained there; and 2.) when an

<sup>53</sup> Medinaceli to Alba, Bruges, 15 June 1572, *Codoin*, 36:42; Mendoza, *Comentarios*, p. 454. Emanuel de Meteren mistakenly wrote that the Lisbon fleet reached Flushing after the Vizcayans reached Rammekens. *L'Histoire des Pays-Bas. . . ou recueil des guerres et choses mémorables* (The Hague, 1618), 73v. Merchandise from the Lisbon ships provided valuable capital for the Dutch at the start of their rebellion. Unlike the wool fleet, the fleet from Lisbon has attracted the notice of historians. See Wegg, *Decline of Antwerp*, 157.

<sup>54</sup> Julián Romero to Zayas, Brussels, 24 June 1572, *Codoin*, 75: 59–62. The arrival of the ships at Rammekens was reported by watchers on the towers of the castle of Sluys, *Codoin*, 75:53–56, and by three citizens of Bruges, including the *Burgomeister*, who watched from the Flemish coast until they were chased away by local workmen. *Codoin*, 36: 83–84. See also ARCV, *Masas*, leg. 261, *caja* 1048–1.

<sup>55</sup> A full account of the fleet's experiences in the Scheldt appears in questionnaires prepared by the shippers and insurers in support of their opposing legal briefs. There are numerous copies in ARCV, *Masas*, legs. 260–61. Here we have used one insurers' inquiry (dated Burgos, 21 November 1577, *ibid.*, leg. 261, *caja* 1048–1) and one shippers' inquiry (dated Valladolid, 14 December 1577, *ibid.*, leg. 260, *caja* 1043–1). Selma Barkham provided information about the impressment of the *León Rojo*.

<sup>56</sup> The events leading to the siege and surrender of Middelburg are discussed in most of the standard histories.

insured voyage ended, whether at the destination stated in the policy or when the wool was unloaded and the crews dismissed.

Equally important to the case were the actions and attitudes of members of the Burgos *Consulado*. For a while they simply waited, hoping that the turmoil in Zeeland would soon blow over. Then, with Middelburg under siege, the prior and consuls of the *Consulado* wrote the king on April 17, 1573 requesting immediate aid.<sup>57</sup> We have not found Philip's reply, but from their second letter it is clear that he reviewed Middelburg's situation and asked the prior and consuls for advice. Meeting to discuss the affair, members of the *Consulado* were deeply divided. Fifteen were shippers as well as insurers. They wanted the wool sent on to Bruges, regardless of the risk. The majority, however, was afraid to move it, lest it be captured by the rebels. As a body, the *Consulado* could not give the king any advice. Its members could not even honor his request for a simple accounting of all the wool involved, presumably because they did not know.<sup>58</sup>

Once Middelburg fell, the shippers began to file insurance claims, but from the beginning the insurers refused to honor their policies. Normal procedures for disputed claims required the insured party or his agent to present documentation before the prior and consuls of the *Consulado*, who would judge the dispute according to established ordinances. The Middelburg claims, however, were transferred to the royal Chancellery in Valladolid late in 1574, over the protests of the prior and consuls.<sup>59</sup> Manuel Basas Fernández, the most recent historian of the *Consulado*, could not understand why this happened, since the *Consulado* routinely decided cases against insurers, and—so Basas thought—the losses were not exceptionally large.<sup>60</sup> The change of venue is perfectly understandable, however, given the judges' conflict of interest. Andrés de Polanco, the prior, was both an insurer and a shipper in the case, as was one of the two consuls, Sancho de Agurto,<sup>61</sup> who had also lost wool in Middelburg that he had shipped from the Andalusian port of Sanlúcar de Barrameda.<sup>62</sup> Only the remaining consul Melchior de Astudillo does not appear to have been either a plaintiff or a defendant in the Middelburg case.

In Valladolid the judicial process moved slowly. We have not found records for the initial proceedings, but references in the appeals indicate that the four judges hearing the case found for the shippers on July 30, 1577.<sup>63</sup> The insurers lost no time in filing an appeal through their representatives Diego de Curiel and Francisco de Maluenda. The shippers' representatives were no less distinguished: Sancho de Agurto, Cristóbal Suárez, and Luis de San Millán.

<sup>57</sup> ARCV, *Masas*, leg. 260, caja 1042-1, fol. 190v.

<sup>58</sup> *Ibid.*, fols. 191r-191v.

<sup>59</sup> Letters from the *Consulado* to the king, cited in Basas, *Consulado*, 116, 124.

<sup>60</sup> *Ibid.*, 116. Several critical components of the affair seem to have escaped Basas. He misjudged its magnitude, for one thing, reporting only forty claims in one place and "more than thirty" in another, against "more than one hundred" insurers. (*Ibid.*, 115-16, 124).

<sup>61</sup> ARCV, *Masas*, leg. 261, caja 1047-1.

<sup>62</sup> *Ibid.*, leg. 260, caja 1041-1.

<sup>63</sup> *Ibid.*, caja 1024-1, fols. 235r-236r. The judges were all *licenciados* (having advanced university degrees), named Arpide, Hinojosa, Ybáñez de Valmaseda, and Valcárcel.



Agurto, we recall, was one of the consuls of the Burgos *Consulado*, as well as being a shipper and an insurer. Despite these contradictory loyalties, his prestige and experience in commercial law made him a powerful ally. San Millán and Suárez were both Segovian citizens who had sent hundreds of woolsacks with Medinaceli's armada.<sup>64</sup>

In the first stage of the case, the judges apparently based their decision on the insurance policies alone. The later appeals called for additional and more varied proofs. The king authorized both sides to draw up *probanzas* (questionnaires) and interrogate witnesses of the events, setting a time limit of 120 days from November 15, 1577 for their completion.<sup>65</sup> Neutral and reliable notaries administered the questionnaires in Bilbao, Burgos, Laredo, Portugalete, Ses-tao, Santander, Madrid, Seville, Castro Urdiales, San Sebastián, Fuenter-rabía, Pasajes, Deva, "and in other towns and places of the province of Guipúzcoa and of these kingdoms."<sup>66</sup> One was even administered in Bruges.<sup>67</sup> Four notaries in Burgos and one in Portugalete were specifically forbidden to participate because of a possible conflict of interest,<sup>68</sup> but otherwise local notaries acted as agents of the court. Shippers and insurers each issued separate questionnaires of their own design. The king ordered only that each witness be asked his age and any interest he had in the case. After examining many of these questionnaires in the Chancellery, we concluded that the answers are unimportant, since they seldom involved more than an elaborate "yes" or "no." It is the questions that reveal the line of argument taken by each side.<sup>69</sup>

The insurers concentrated on defining an insured voyage. They began by asking witnesses if they knew about Medinaceli's armada and specifically about the eight principal ships. Then they reviewed the events in Zeeland as they interpreted them. Several questions tried to establish that the unloading in Middelburg by Ballo and Arnedo had been done in an orderly and legal fashion. Once Castro de la Loo, the agent of the Bruges consuls, formally received the wool and warehoused it in Middelburg, the voyage was finished. That the ships' captains had received freighting payment and had then paid off their crews was proof of this. The key element in their case appeared in question fourteen, which asked the witness to affirm that Burgos insurers customarily covered risks only at sea, not on land. Within living memory, they had never done differently. Thus the insurers' risks had ended when the wool was unloaded for Middelburg. Unexpectedly, the insurers then weakened their case by taking an interest in the wool thereafter. Why had it been kept in Middelburg so long? Owners of other merchandise had removed their goods safely before the rebel siege; the wool shippers could have done the same.

The shippers' questionnaire concentrated on the circumstances that caused

<sup>64</sup> ARC.V, *Cartas ejecutorias*, leg. 1398; *ibid.*, *Masas*, leg. 260, caja 1045-1.

<sup>65</sup> ARC.V, *Masas*, leg. 260, caja 1045-1.

<sup>66</sup> *Ibid.*, leg. 261, caja 1048-1.

<sup>67</sup> *Ibid.*, leg. 260, caja 1044-1.

<sup>68</sup> *Ibid.*, leg. 261, caja 1048-1.

<sup>69</sup> For the insurers we used a questionnaire taken in Burgos, 21 November 1577, *ibid.*; for the shippers a questionnaire taken in Valladolid, 14 December 1577, *ibid.*, leg. 260, caja 1043-1.

the wool to be warehoused in Middelburg and held there. When ships ran into difficulties before reaching the end of the voyage and were forced into unusual ports, they asked, was it not customary for the consuls of Bruges to send agents to store and later expedite the wool when practical? Was it not also customary that the insurance was in force until the designated port was reached? On behalf of the Castilian consuls of Bruges, Juan Castro de la Loo had gone to Middelburg and discovered that the captains of the armada would not let him take the wool to Bruges. It could not be disposed of elsewhere, because, according to a privilege of Charles V, wool consigned to Bruges could be sold in no other place.<sup>70</sup> Castro de la Loo had again sought a license from Medinaceli and the other authorities to take the wool to Bruges, and they had refused, fearing rebel action. Others who had tried to move goods were detained by the authorities.

Getting to their main argument, the shippers asserted that they could not have moved the goods because of official refusals, and, if they had illegally done so, the insurers would have acted against them. Their evidence on this point was the fate of Andrés Gutiérrez, a native of Burgos who removed some woolsacks from Middelburg by water to Antwerp and then by land in hopes of reaching Bruges. On the road enemy troops confiscated his wool. His insurance was paid, but later the insurers sued him before the *Consulado* and obtained a judgment against him. Gutiérrez had to return the money he had received and pay a fine of twenty percent, since, in the *Consulado*'s judgment, he had no right to move the wool from Middelburg. The closing questions indirectly accused the insurers of enriching themselves by investing the money they legally owed to the shippers.

We do not know the precise points the royal judges used in reaching their decision, but there were several things in the shippers' favor. On the basis of the policies themselves, the law was on their side. The ordinances of the *Consulado* governing insurance exist in two versions, one in force from 1538 and a new set drawn up and adopted in 1572.<sup>71</sup> In the ordinances and in the policy forms coverage was clearly in force from land to land, "*de tierra a tierra*," from the time the goods were loaded until they were unloaded at their destined port.<sup>72</sup> Thus the completion of the voyage was not simply the physical unloading; it had to be done at the port of final destination. The policies covered losses caused "by sea, wind, fire, friends, enemies, reprisals . . . , or detention by a king, lord, or other authorities."<sup>73</sup> Contemporary events were not lost on Philip II and his advisers; in April 1572 (too late, incidentally,

<sup>70</sup> Confirmations of this privilege appear in Louis Gilliodts-Van Severen, ed., *Cartulaire de l'ancienne estaple de Bruges: Recueil de documents concernant le commerce intérieur et maritime . . .* (4 vols., Bruges, 1904-06), 3:212, 236, 242.

<sup>71</sup> The 1538 ordinances are presented in Eloy García de Quevedo y Concellón, *Ordenanzas del Consulado de Burgos de 1538 que ahora de nuevo se publican, anotadas, y precedidas de un bosquejo histórico del Consulado* (Burgos, 1905). The 1572 ordinances exist in a very rare printed volume, published in Burgos in 1572. One copy is contained in ARCV, *Masas*, leg. 260, caja 1042-1.

<sup>72</sup> For the 1572 policy form, see García de Quevedo, *Ordenanzas*, 237n. Since 1542 a policy form approved by Charles V was in use, very like the 1572 form. *Ibid.*, 236n.

<sup>73</sup> *Ibid.*, p. 238n.



to be reflected in the policies in the Middelburg incident) the king ordered that no insurance could be written covering actions of enemies or corsairs.<sup>74</sup> The insurers had argued that the voyage was completed when the goods were unloaded in Middelburg and the captains of the large ships were paid off, but the wording of their policies specified that the risk was run until the wool reached the port of Bruges. Walcheren Island was the customary lightering point for goods going to the interior of Flanders and Brabant, especially to Antwerp and Bruges,<sup>75</sup> but detention in Middelburg was clearly against the will of the shippers and the designated receivers of the wool in Bruges.

The insurers' insistence that the goods could have been moved freely from Middelburg to Bruges any time before February 1574 was especially ironic in view of their own actions. On March 10, 1573 the prior and consuls had called the *Consulado* into session to discuss a letter from the Castilian consuls in Bruges, who reported that a fleet had been sent to Middelburg to aid the garrison there. On its return, the Bruges consuls urged, it could take some sacks of wool to Antwerp or Bruges. Present to consider a reply were fifteen members of the *Consulado*, thirteen of whom were insurers in the case. The remaining two, Juan del Lago and Francisco del Peso, were involved as both shippers and insurers. The outcome of the meeting was a resolution informing the men of Bruges "that the sacks be left there in Middelburg and not taken from there until the land was pacified and they could be shipped without danger."<sup>76</sup> From Bruges the consuls responded that they still thought it would be best to move the wool. The Burgos *Consulado* met again on May 4, 1573 to reconsider. Seventeen members were present, seven of whom had been at the meeting in March. Francisco de Castro Múxica, an insurer, took the floor to urge that the sacks not be moved. He did not want to be obligated, he said, to anything beyond the terms of the policy. He was supported by five other insurers. Though six men were present who had both shipped and insured wool and one man who was solely a shipper, the insurers carried the day. They again sent the same advice to Bruges.<sup>77</sup> Their actions on these two occasions show that the insurers still felt responsible for the wool and that they could prohibit its removal from Middelburg. As we have seen, they even sued one shipper who moved his wool. This made nonsense of their later claims.

On April 7, 1579 the judges of the Chancellery again found in favor of the shippers.<sup>78</sup> The insurers were sentenced to pay off in full on the policies, plus court costs of 1,500 *doblas*. Again the insurers appealed, in a brief filed on April 25. First of all they pointed to irregularities in the timing and form of the shippers' cases. The rest of their argument followed familiar lines. Putting the

<sup>74</sup> Royal pragmatic of 30 April 1572, Madrid, cited *ibid.*, 242n. This rule was embodied in the 1572 ordinances as ordinance 82.

<sup>75</sup> Bindoff, *Scheldt Question*, 39, 63.

<sup>76</sup> ARCV, *Masas, Fenecidos*, leg. 260, fols. 203-04, cited in Basas, *Consulado*, 274-75. Basas has a peculiar method of citation for documents in ARCV. Neither we nor the archive staff could find the documents cited here or in the next footnote, yet there is no reason to doubt their authenticity.

<sup>77</sup> ARCV, *Masas, Fenecidos*, leg. 260, fols. 204v-205, cited in Basas, *Consulado*, 275-76.

<sup>78</sup> ARCV, *Masas*, leg. 260, *caja* 1042-1, fol. 763v.

wool ashore terminated the policies, which were only for the sea voyage and had nothing to do with risks run on land. The insurers still refused to admit that the authorities of the armada had forced the wool's unloading or that fear of the enemy had been sufficient to cause the wool to be put in Middelburg. They sarcastically noted that although the wool was taken to Middelburg for safety the Prince of Orange took the city anyway! They also persisted in the assertion that the wool could have been moved from Middelburg, omitting any mention of their own actions to prevent it. In desperation, they even protested the court costs, complaining that the shippers had not had to pay costs in the first stage, and that, in any event, both sides of the case had merit.<sup>79</sup> Their own appeal, they went on, had covered 6,205 sheets of paper and had already cost them 24,820 *maravedis* (about 66 ducats).<sup>80</sup>

The judges and the king reviewed the case and issued the definitive sentence against the insurers on June 3, 1579.<sup>81</sup> On July third the president and judges of the Chancellery did exempt the insurers from paying off on thirty-eight sacks of wool which had been saved and sold.<sup>82</sup> For the rest, the shippers were instructed to come to Valladolid to have *cartas ejecutorias* (writs) prepared, specifying that their claims had been granted and giving them the right to collect from the insurers.

That was easier said than done. One pathetic episode shows how doggedly the insurers fought compliance with the verdict and how severely the blow was felt in Burgos. One of the shippers, the Segovian Andrés Moreno, had been awarded a judgment of 5,850 ducats on 187 sacks of wool. When he could not get his writ honored by the insurers, he complained to a judge of the Chancellery, the *licenciado* Gil Cobos Bermúdez. Cobos went to Burgos to see justice done, and his affidavit offers a fascinating glimpse into the activities of the court as an enforcing agency. The judge sequestered the goods of insurers Juan Gómez de Aguayo, Jerónimo López, Lope de Gaona, and Gaspar de Vallejo. The first three were also rounded up by uniformed constables, and, since no one would act as their bailbondsman (*fiador de saneamiento*), they were imprisoned for ten days each. Gaspar de Vallejo had fled, possibly hiding in a wooden wardrobe; the judge settled for impounding several of his houses instead.<sup>83</sup> This episode amplifies the note of desperation that ran through the insurers' final appeal. Even when faced by jail, at least four insurers would not, or could not, pay either Andrés Moreno or their own bail. More telling is that no one would advance bail for them.

By the 1580s Burgos was finished as the insurance and commercial center of

<sup>79</sup> *Ibid.*, fols. 560r–578r.

<sup>80</sup> *Ibid.*, leg. 261, *caja* 1048–1.

<sup>81</sup> *Ibid.*, leg. 260, *caja* 1042–1, fols. 622r–624r.

<sup>82</sup> *Ibid.*, fol. 634r.

<sup>83</sup> Testimony of Gabriel Navarro, an official of the Chancellery, made in Burgos 1 February 1580. *Ibid.*, *caja* 1041–4. We have not been able to determine when and in what amount the insurers paid off. The Chancellery records contain only the writs authorizing the collection, and the extant books of the *Consulado* (located in the Diputación of Burgos) do not cover the period in question. We have assumed that the claims were eventually paid.

the wool trade,<sup>84</sup> and the few Burgalese who survived as important businessmen and financiers did so as individuals, not as members of a flourishing commercial community.<sup>85</sup> By the 1590s Madrid surpassed Burgos in insurance underwriting, as it surpassed Medina del Campo as a financial clearing house.<sup>86</sup> The many reasons for this shift are beyond the scope of this article, and certainly the losses in Middelburg were not alone responsible for it. Even so, there is good reason to see the Middelburg incident as a decisive blow to Burgos, because of its magnitude and its timing.

From 1569 to 1572, when Medinaceli's armada was the only significant sailing, Burgalese merchants shipped 9,210 sacks of wool to Flanders,<sup>87</sup> and 1,579 of these were lost in Middelburg. By then, the revolt had already disrupted the Flanders trade; just before the revolt, from 1561 to 1568, Burgalese merchants had shipped an average of 5,789 woolsacks annually to Flanders.<sup>88</sup> We can use the losses of Burgalese merchants in Middelburg (25.6 percent of all the sacks lost) and their total shipments from 1569 to 1572 to estimate that 35,976 woolsacks altogether were sent from Spain to Flanders in that period. The Middelburg woolsacks were therefore about 17.1 percent of the total shipment. These were not extraordinary losses for a trade that was risky at the best of times, but they came after a steep decline in the wool trade, and they fell particularly hard on Burgos.<sup>89</sup> Over forty percent of the insurers involved in the Middelburg case were from Burgos, and over twenty-eight percent of the shippers. The insurers had to pay claims that were worth four times the annual *alcabalas* tax for the whole province of Burgos.<sup>90</sup> The shippers lost operating capital while their wool was stored in Middelburg, and the insurance, when they finally collected it, still did not compensate them for lost profits. These substantial losses were magnified in Burgos because they fell on a commercial community that was already weakened.

This one case clearly illustrates the hazards to which Spanish commerce was subject in that turbulent age. Even before the armada arrived in Flanders, a number of ships had been lost to ordinary misfortunes that had nothing to

<sup>84</sup> Manuel Basas Fernández, *El seguro marítimo en España del siglo XVI* (Bilbao, 1963), 20–23. Insurance policy registers for the late sixteenth century show a rapid falling off of business. The last register from 1594 to 1619 trails off into blank pages about one-third through the book. Archivo del Consulado de Burgos, especially items 28 and 35.

<sup>85</sup> Some of them can be traced in Vázquez, *Lettres marchandes d'Anvers*; Felipe Ruiz Martín, *Lettres marchandes échangées entre Florence et Medina del Campo* (Paris, 1965); José Gentil da Silva, *Marchandises et finance. Lettres de Lisbonne 1563–1578* (2 vols.; Paris, 1959); and José Gentil da Silva, *Stratégie des affaires à Lisbonne entre 1595 et 1607. Lettres marchandes des Rodrigues d'Evora et Veiga* (Paris, 1956).

<sup>86</sup> Felipe Ruiz Martín, *Lettres marchandes échangées entre Florence et Medina del Campo*, cxlv; Vázquez, *Lettres marchandes d'Anvers*, 1: 16; Basas, *Consulado*, 260.

<sup>87</sup> Basas, *Consulado*, 259, citing AGS, *Diversos de Castilla*, leg. 10, fol. 11.

<sup>88</sup> *Ibid.*

<sup>89</sup> Lodovico Guicciardini said Spain sent 40,000 sacks of wool each year to Flanders in the first half of the sixteenth century and that after 1560 exports fell to approximately 25,000 sacks. Unfortunately, he gave no source for his figures. Cited in Jules Finot, *Étude historique sur les relations commerciales entre la Flandre et l'Espagne au moyen âge* (Paris, 1899), 250–51. According to the *Consulado*, Burgos merchants, who in about 1550 had shipped between 65,000 and 70,000 woolsacks per year to Flanders, France, and Italy, shipped only 20,000 to 25,000 sacks in 1582. Since the *Consulado*'s object was to protest high taxes in 1582, we must use the figures cautiously, but they are probably more accurate than other estimates. Basas, *Consulado*, 259.

<sup>90</sup> Ulloa, *Hacienda real*, 132.

do with the Dutch revolt. To absorb those losses and potential damage by the rebels, the insurers of Burgos had already increased their premiums from the mid-century norm of five percent to from nine to thirteen percent after 1569.<sup>91</sup> Philip's 1572 decree that merchant ships could not be insured against enemy action at any price created another impediment to the Flanders trade.<sup>92</sup> The circumstances surrounding the Middelburg losses were unprecedented in their legal complexity and threw accepted standards of maritime insurance into disarray. The disputed claims split the Burgos *Consulado* and eroded the confidence of shippers and insurers alike in the continued viability of northern commerce. Despite these difficulties, the Flanders wool trade did not stop, but after Middelburg most Spanish wool shipments to Flanders arrived overland, after being unloaded in the French ports of Nantes, Rouen, Calais, and Dunkirk.<sup>93</sup> This complicated route added transit costs, tolls, and the uncertainty of passage through France to an already expensive and risky enterprise.

<sup>91</sup> Basas, *Seguro marítimo*, 111, states that insurance rates rose to 9–10 percent in 1569, but we found that most policies for the Middelburg voyage were written at 12 percent, with some as high as 13 percent.

<sup>92</sup> Royal pragmatic of 20 April 1572, Madrid, cited in García de Quevedo, *Ordenanzas*, 242n.

<sup>93</sup> Basas, *Seguro marítimo*, 112; Archivo del Consulado de Burgos, item 28; See also Pierre Jeannin, "Aspects du développement capitaliste dans les milieux d'affaires de Rouen à la fin du 16<sup>e</sup> siècle," *Fourth International Conference of Economic History*, Bloomington, 1968 (Paris, 1973), 91–94.

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## Reviews of Books

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### GENERAL

TRAIAN STOIANOVICH, *French Historical Method: The Annales Paradigm*. Foreword by FERNAND BRAUDEL. Ithaca: Cornell University Press. 1976. Pp. 260. \$12.50.

Traian Stoianovich's book summarizes the work of a group of historians—led by Fernand Braudel—more or less identified with the French journal *Annales: Économies, Sociétés, Civilisations* since World War II. The *Annales* group has been influenced by the issues and techniques labeled “structuralism.” And indeed Stoianovich speaks of “communications and exchange,” “message systems,” “general communications theory,” “the communications paradigm,” “the historian's structuralism,” “system of signs,” and so on. “As a result of *Annales* research,” he says, “. . . we are able today to explain a whole society and its message systems and to define structure, conjuncture, function, and event far better than historians of the era before 1946” (p. 235). His summary claim is far-reaching: “that the total effect of *Annales* inquiry . . . has been to create an historical paradigm of ‘disciplinary matrix’ for the world community of historical scholarship” (p. 236).

Stoianovich sees this paradigm crystallized in something Braudel calls “a conception of history in terms of ‘*n*-dimensions’ ” (p. 46). He proceeds to document exhaustively how the *Annales* paradigm has entertained a number of exotic strategies: serial history, functional history, *histoire globale*, communications analysis, and quantification techniques. Admitting that some have seen this flexibility as confusion, Stoianovich argues convincingly that the various aspects of *Annales* historiography are complementary. Here he is at his best. For the elusive term “paradigm” does suggest a family of related but distinct approaches (its current usage derives from Wittgenstein's language-games through the work of T. S. Kuhn), and the methods of the *Annales* school do seem to constitute a diverse set of tools marked by poten-

tially unifying, abstract, quasi-mathematical affinities.

But interesting as this may be to structuralism buffs, it is hard to see any justification for the exaggerated claims of professional priority Stoianovich makes for his heroes. (In his foreword, Braudel himself is somewhat more cautious.) Stoianovich uses the notion of a disciplinary paradigm aggressively to advance the privileged status of certain methodologies. Ironically, however, this notion has come to stand for a methodological relativism in philosophy of science, where it was first employed. Stoianovich suggests tolerance, but professional provincialism and pretention lurk in the background.

It is curious that the author's own historiographical techniques seem to owe little to the school he describes and to which he professes an allegiance. He unfolds the *Annales* story in terms of a gratuitous, three-stage historiographical theory: the *Annales* paradigm itself, and the earlier, now outdated paradigms of what Stoianovich calls “exemplar” [*sic*] and “developmental” history. But even worse, this history of the *Annales* school is glued together not by “*n*-dimensions” but by the familiar techniques of the history of ideas with all their outmoded “exemplar” and “developmental” strategies. The contradiction between Stoianovich's own manifest historiographical techniques as used in his description of the school he advocates, and the techniques propounded by that school itself as he reports them, may be left to the intelligent reader to ponder.

ADRIAN KUZMINSKI  
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CARL G. GUSTAVSON, *The Mansion of History*. New York: McGraw Hill. 1976. Pp. ix, 403. \$4.95.

Although Carl G. Gustavson does not quite tell us what his purpose is in producing the volume before us, it appears that what he has in mind is a textbook to introduce beginning students to the study of history. One may presume, then, that he will be

disappointed to discover that his book has been turned over to a professor of philosophy for review. What could I be expected to know about such textbooks, and how could I appreciate the ways in which the volume under review improves upon its predecessors—for the author must surely believe that it does. I did not know that the history preparation of high-school graduates is so poor as to require so elementary an introduction to so many basic and simple ideas as this book contains until I consulted a member of my university's history department, who assured me that such is the case.

Even so, I confess that the gimmickry which sometimes mars the pages of the book—particularly early on—is grating and suggests a bit of condescension toward beginning students. Surely, they can be introduced to the study of history without such silliness as dictators named “Won Ahl de Marbles” and chapters called “Luther Hits the Jackpot . . .” As the book proceeds, Gustavson gives up that sort of nonsense and writes more seriously. It seems to me that he manages to take into account most—perhaps all—of what enters into historical-mindedness, which is what he wants to inculcate in his readers. Obviously, it is not possible for him to do real justice to any of the many themes he takes up; he can do little more than indicate the various considerations that enter into thinking historically, and point in a general way to different views which have been taken toward whatever a given chapter deals with.

In view of the character of the book, it would not be fair to criticize the author for not dealing adequately with this or that issue. But, I would like to note briefly that I am surprised to see a paradigm, in Kuhn's sense, called a “general statement of relationships” (p. 223); the origin of modern historical scholarship traced to the seventeenth century (p. 257) six years after the appearance of the books of Kelley and Huppert on sixteenth-century French historiography; Hegel called a systematizer of romantic ideas of historical change (p. 260), since his views are decidedly unromantic and he was a biting critic of that movement; and the identification of interest in generalizations with subjectivism and interest in concrete fact with objectivity (p. 315).

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PAUL JOHNSON. *A History of Christianity*. New York: Atheneum. 1976. Pp. viii, 556. \$13.95.

Can a scholarly history of Christianity ignore Tillich, Bultmann, Kierkegaard, Aquinas, modern Ireland, the “death of God” movement, liberation theology, the iconoclastic controversy, Vietnam

and the churches, the early Catholic missions in North America, and the relation between missions and ecumenism, among other subjects? One is tempted to say no. But this is no standard history in search of a plot. It is time, says Paul Johnson, to balance the books on the “institutional philosophy” called Christianity. He does his calculating openly and with vigor.

His plot revolves around three persons: Paul, Augustine, and Erasmus. Paul's militant universalism saved Christianity from narrow sectarianism and extinction, and Jerusalem's destruction ensured that the gravitational center shifted to Rome. Augustine, the “dark genius of imperial Christianity,” saw compulsion as part of the total Christian society. Riding the crest of the New Learning, Erasmus rejected “mechanical Christianity” and pursued a minimal theology based on peace and unanimity; his program of moral reform connected faith and truth, imposed no uniformity, and forced no conscience.

According to Johnson, the Protestant Reformation reduplicated the authority-prone, society-controlling form of Augustinian Christianity that it opposed in Roman Catholicism. On the other hand, the Erasmian “third force” had much in common with progressive economic elements, opposed religious wars, and tried to synthesize theology and natural science. The Erasmian vision informed Christianity in America with its religious liberty.

One might easily quibble over details. Does Protestantism owe its survival to the Turks (p. 283)? Was Wesley's Christianity “almost totally devoid of intellectual content” (p. 365) and Aquinas merely a “pundit” (p. 464)? Are Southern Baptists really “segregationalist” (p. 499)? But there are more substantial problems. Johnson credits Augustine's theology too much and Constantine's earlier legalization of Christianity too little. He passes over the Cluniac reform movement that turned Gregory into a reformer and conveniently ignores Aquinas' brilliant response to Aristotelian secularism. He fails to define the frequently used terms “millennial” and “millennium” and describes the Catholic Reformation simply as the Counter Reformation. Nineteenth-century German Christianity is largely irrelevant for him except for the *Kulturkampf*; his description of Schleiermacher does not account for his continuing influence. Johnson discusses American Christianity all too briefly, and his history of the church after 1800 reads much like a history of the papacy. With only passing interest in Byzantium and largely ignoring Russian Orthodoxy, the book might appropriately be titled “Western Christianity—Its Eastern Origins and Expansion.”

Still this is a good book, one that successfully summarizes the “published results of a vast



amount of research that has been conducted during the past twenty years" (p. vii). It shows the intimate relationship between Christianity and Western society most masterfully when it describes monasticism's economic importance and Victorian evangelicalism. It is necessarily selective, but each chapter sparkles with vignettes of important figures. The style is impeccable and almost racy, laced with quotable quotations. Satire and irony are used effectively. The study is a startling introduction for the neophyte and a stimulating synthesis for the specialist. The author holds to an Erasmian vision of Christianity whose primary purpose is not to create dynamic societies "but to enable individuals to achieve liberation and maturity in a specific and moral sense" (p. 516). This view gives his pen a cutting edge that makes the history of Christianity readable and, for the Christian, often regrettable.

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STEPHEN SHAROT. *Judaism: A Sociology*. New York: Holmes and Meier, 1976. Pp. 224. \$13.50.

Clumsily written and sloppily edited, Stephen Sharot's *Judaism: A Sociology* is difficult to read and unreliable. At its best, the book rehashes well-worn concepts, sometimes borrowed from historians and warmed in a sauce of sociological jargon. At its worst, the book is replete with errors, misleading statements, tautologies, and poorly defined and inconsistently used terms. The number of typographical errors is higher than average; there is no bibliography.

Here are just a few of the many errors and misleading statements: Sharot states that there are 500,000 Jews in Argentina, although recent studies have shown that there are only 300,000, and that levels of affiliation are consequently higher than previously thought. He states that the central consistency of France never dismissed a rabbi, although, in fact, it did. He mistakenly believes that the grand rabbi of France was the head of the rabbinic school. He assumes that the hierarchical Jewish organization of France was based on Catholic models, when in fact it was based on Protestant models.

Sharot's use of language is exasperating. He uses the term "acculturation" in different ways throughout the book. To the term "marginal Jew" he applies a different meaning from that commonly accepted. The eccentricity and poor quality of writing throughout the book are typified by the following example (p. 55): "In this, Holland, like other European countries, differed from England and America, being Jews organized in voluntary associations not autonomous communities."

Basing himself on the many published works of sociologists and social historians who have studied problems of acculturation, assimilation, and Jewish identity, Sharot asserts that his personal contribution has been to supply a comparative framework. He expects, thereby, to discover the general rules governing the cause-and-effect relationship between socioeconomic conditions and changes in Jewish behavior. The tautological nature of his generalizations, however, is exemplified by his statement (p. 72, and rephrased on p. 123): "The traditional Jewish community continued longest where Jews remained separated from non-Jewish society and where the Jewish community continued as a powerful agent of social control."

PHYLLIS C. ALBERT

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SALO W. BARON, ARCADIUS KAHAN *et al.* *Economic History of the Jews*. Edited by NACHUM GROSS. New York: Schocken Books, 1975. Pp. xii, 304. \$15.00.

Can an economic history covering several thousand years and numerous diverse geographical areas be contained in one small volume? The book under review provides an ambiguous answer. Based on the brief introduction for which Nachum Gross was responsible and the fifty pages written by Arcadius Kahan on the period from about 1500 to the present, the answer could be positive. These portions of the book are characterized by well-articulated theoretical questions combined with highly competent answers based on thorough research. The rest of the book tends to be considerably more descriptive—which is a more than respectable method of historiography—but it also succumbs frequently to the episodic and anecdotal. If the latter methods are primarily used, then a one-volume effort can hardly do justice to the subject.

The book is divided into four parts. The first is devoted to a general survey from the time of the First Temple to today, a most ambitious effort indeed. The other three parts take up agriculture, industry, and the service sector, with several sub-topics in each category. Of this group of topics I found the nearly twenty pages on agriculture the most interesting. It serves a useful purpose to be reminded that in a nonindustrialized, nonurban world, Jewish economic activity was closely tied to agriculture although Jews themselves were rarely agriculturists after the eighth century A.D. This was the case not only before the eighteenth century, when England and then the rest of Western Europe began to industrialize, but even up to the First World War in Eastern Europe. In these sections of the book there is a large discursive element, fully understandable when so many areas and eras have to be considered. The Near and

Middle East, Spain, Eastern Europe, and modern Israel perforce must all be included when a particular topic is taken up.

At best this book whets the appetite for more. Being composed of articles written for the *Encyclopaedia Judaica*, it presents a fairly broad menu of material. Some of the articles provide new information and/or new insight, but in many cases it is primarily the well-known that is repeated.

HERMAN FREUDENBERGER  
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DAVID C. LINDBERG. *Theories of Vision from Al-Kindi to Kepler*. (University of Chicago History of Science and Medicine.) Chicago: University of Chicago Press. 1976. Pp. xii. 324. \$20.00.

In the field of medieval optics we have no book giving its history. Vasco Ronchi's *Storia della luce* is too spotty and unreliable. Carl Boyer's *The Rainbow, From Myth to Mathematics* gives a detailed and generally dependable account of one important theme in medieval optics. David C. Lindberg's is the first attempt to survey visual theory completely.

At the basis of this book is the assumption that optics in general is a science with a tradition (à la Thomas S. Kuhn). As such, its development comes primarily and almost solely from internal stimuli and problem-solving. Lindberg carries this assumption further to identify visual theory, an essential element within optics, in the same terms. The traditions which he identifies and traces with admirable clarity are mathematical, physical-philosophical, and physiological (or medical). Lest any doubt remain about self-awareness, Lindberg speaks of the "practitioners of visual theory" (p. x). Tracing the three traditions from a brief consideration of Greek roots, the author branches out impressively to cover all major writers in the Arabic tradition to the twelfth century and then traces the Latin tradition through the Middle Ages and through Renaissance perspective painting to Kepler.

Alhazen (ca. 965–1039) is clearly the hero of the narrative. In him the virtues of each tradition are captured in a theory which is primarily physical, yet uses the vocabulary, methods, and achievements of the other traditions as well. Alhazen's application of radiation from point sources and his establishment of one-to-one correspondence between points on an object and in the eye are essential contributions that lie at the base of Kepler's theory of vision. Kepler becomes the apex of medieval tradition rather than a mechanical philosopher of the Scientific Revolution.

In developing the narrative, Lindberg analyzes the texts of every writer with care and in detail. The analysis is so meticulous that the reader will

repeatedly feel almost overwhelmed with accuracy. This is not only careful scholarship but painstaking documentation of the thesis that visual theory (and presumably optics in general) is an enterprise that advances through the logic of internal problem-solving, not the enthusiastic application of "light-metaphysics" or some other external influence. Lindberg's essay in the history of ideas is essentially unimpeachable in its details—an excellent study of texts and theories—and can be attacked only by dismantling the assumptions involved. That undertaking is a matter of interpretation, more appropriate in other places than here.

BRUCE EASTWOOD  
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ANNA MARIA BATTISTA. *Lo spirito liberale e lo spirito religioso: Tocqueville nel dibattito sulla scuola*. (Di fronte e attraverso, number 3.) Milan: Jaca Book. 1976. Pp. 201. L. 3,500.

Doris S. Goldstein's *Trial of Faith: Religion and Politics in Tocqueville's Thought* (1975) presented the first extended discussion of religion in Tocqueville's historical, political, and sociological thought. Anna Maria Battista, professor of the history of political thought at the University of Rome, has now added a work which complements Goldstein's in thoroughness and analytical care. The emphasis is on a detailed scrutiny of Tocqueville's convictions and actions when he entered the intense debate that preceded and surrounded the Villemain bill of 1844 which was intended to strengthen the University's regulation of private secondary education. This was a very unhappy experience for Tocqueville. It provoked the scorn of liberals, the distrust of Catholics, and endangered his friendship with Gustave de Beaumont who had been his companion on the famous voyage to America.

Battista seeks to explain why Tocqueville acted as he did and to uncover the logic of his historical and sociological perspective which separated him from both liberals and clericals. She confesses that despite herself in the course of her work she only reluctantly concluded that Tocqueville viewed religion from a traditional theological position, even a conservative one. Personally agnostic, he considered religion an affair of heart and soul, of dogma beyond the reach of conventional rationality. From this circumscribed sphere, religion, the basis of morality and culture, was obliged to leave to politics the dreams of reason appropriate to the search for a free society. Any violation of the restricted autonomy proper to religion or to politics was necessarily disastrous to the prospects of an open society. From the vantage point of the political and religious dialogue in contemporary Italy, Battista

offers the stimulating suggestion that for Tocqueville and for Catholic Communists in Italy today religion is not an "external" reality to be accepted or rejected, but "è un elemento *interno* alla sua *visuale politica e, come tale, ineliminabile*" (p. 36). Tocqueville's pluralism, confusing to his friends and foes, strengthened, she argues, his own liberalism, which allowed no compromise of the rights of individuals or minorities confronting the power of the State.

Two valuable appendixes reprint in French and in Italian translation Tocqueville's significant speech of January 17, 1844 in the Chamber of Deputies and six articles in *Le Commerce* on the education question. Battista attributes "without doubt" on the basis of style and content these unsigned articles to Tocqueville's pen. This brief, elegantly constructed book is a welcome addition to Tocqueville scholarship and a splendid illustration of the quality of recent Italian writing on Tocqueville, his nation, and the history of liberalism.

EDWARD T. GARGAN  
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BARBARA SALERT. *Revolutions and Revolutionaries: Four Theories*. New York: Elsevier. 1976. Pp. ix, 161. \$9.50.

The first obstacle to be overcome in this evaluation of four theories of revolutions is lack of scholarly agreement on a definition of the object of inquiry, here stipulated to be "structural change in a society" (p. 8). The second, which the author ignores, is that three of the four theories do not purport to deal with revolution so defined. Olson's rational-choice theory, in *The Logic of Collective Action*, asks why people take part in collective action. This reviewer's *Why Men Rebel* specifies how psychological and social conditions interact to create potentials for political violence. Chalmers Johnson in *Revolutionary Change* explains violent attempts to change social systems. Marx was interested in the transformation of capitalist societies.

The author nonetheless asserts that "the theories under consideration all share the common goal of attempting to explain revolution" (p. 13) and asks how each satisfies three positivistic criteria. One is whether each identifies factors relevant to revolution, second is whether explanations are offered for these causal factors, the third is whether each theory is testable and supported by the evidence.

The criteria are reasonable, which is more than can be said for their application. Discussing Olson, Barbara Salert concludes that rational-choice theory in present formulations cannot explain why

people would participate in revolutions and cannot be tested. Marxist revolutionary theory is said to be "elusive" because it satisfies none of the three criteria well—not surprisingly, since it was not formulated as an empirical theory. Nevertheless Salert concludes that Marxist and rational-choice theories provide the most promising bases for empirical theories of revolution.

A conclusion that seems whimsical in light of the criticisms leveled against the two theories is no less whimsical than the grounds used to dismiss the other theories. For example, my *Why Men Rebel* is said not to specify exact relationships between causal variables and political violence, when in fact it is the only one of the theories to offer formal definitions of all theoretical terms and formal hypotheses about how each is linked to political violence. The author rejects empirical tests of the theory on shaky grounds. Salert has less difficulty in assessing the empirical evidence for the other theories because, as she accurately observes, none has yet been systematically tested.

There is much to be said for an incisive, comparative critique of contending theories of violent conflict. Unfortunately this book botches the job. In its favor, it introduces the novice reader to the basic concepts and arguments of four relevant theories. But its assessments of them are highly suspect.

TED ROBERT GURR  
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ROBERT BLACKKEY and CLIFFORD PAYNTON. *Revolution and the Revolutionary Ideal*. Cambridge, Mass.: Schenkman Publishing. 1976. Pp. 295. Cloth \$9.50, paper \$3.95.

Robert Blackkey and Clifford Paynton, who have already edited a reader on the subject of revolution, once again combine their respective backgrounds in history and sociology to treat the changing concepts of revolution and the "revolutionary ideal." Their project is extremely ambitious: the authors not only devote several chapters to defining and analyzing revolutions, they also trace the development of the modern concept of revolution from the ancient Greeks to the present. In this 270-page book they discuss a multitude of events, including the Protestant Reformation and the English, American, French, Russian, Chinese, Cuban, and Vietnamese revolutions, as well as those in Mexico, Ireland, Bolivia, India, Guinea-Bissau, Algeria, and Chile. They end with an account of the New Left and Herbert Marcuse. Since their working definition of revolution is an "upheaval which calls for a fundamental change in the existing order" the book also analyzes the Nazi regime, but the emphasis throughout is on left revolutions.

A main theme of the book is that while revolutionary models become modified over the years as a result of changing circumstances, the revolutionary ideal derives ultimately from religious sources. Even Marxism, in the authors' view, is essentially a religious doctrine: "the opium of millions of people."

The breadth of topics covered in such a short space unfortunately leads to sketchy treatment of many of the revolutions, especially the early ones in England, America, and France; and omissions abound. Thus Zapata is barely mentioned in the chapter on Mexico; the threat from Kornilov's rebellion in Russia is totally ignored; and America's role in counterrevolutionary activities in Mexico, Cuba, Bolivia, and Chile is not given appropriate consideration. Their format also causes the authors to make numerous unsubstantiated assertions, like their designation of Ho Chi Minh and Castro as "reluctant revolutionaries," or claims of serious disagreements between Marx, Lenin, and Mao. Certain chapters, such as the ones on Africa and Cuba, are of interest, but the one on Vietnam is especially weak. In short, this book makes a contribution to a very large and complicated subject by raising many more questions than it can possibly answer.

LAWRENCE KAPLAN  
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BRUCE MAZLISH. *The Revolutionary Ascetic: Evolution of a Political Type*. New York: Basic Books. 1976. Pp. viii, 261. \$11.95.

Bruce Mazlish draws on Max Weber and Sigmund Freud as his two conceptual fountainheads in an effort to delineate a modern revolutionary type who features a pattern of internal hardness, control, and discipline, as a prerequisite for external certainty and domination of a political movement. Mazlish traces the historical development of religious asceticism with its displacement of the virtues of hard work, energetic activity, and frugality to economic activity. Oliver Cromwell was the historical agent who converted religious asceticism to political revolution. The secular form of the Protestant ethic was Utilitarianism and its prophets were Bentham and the Mills. Some readers will be familiar with Mazlish's recent work on the Mills as exemplars of father-son conflict in nineteenth-century England. From economic activity, suggests Mazlish, ascetism was displaced to the service of revolutionary political activity in figures such as Robespierre, Lenin, and Mao Tse-tung. These men all learned to control or eliminate personal affection and to steer themselves to do whatever was necessary for the good of the revolution.

The author is indebted to Max Weber in his historical use of the ideal type and to Freud for his classic model of the leader who derives his libidinal gratification from his followers rather than from intimate involvement with family, friends, lovers, or other significant personal relationships, which psychoanalysis terms "objects." Mazlish has legitimately abstracted the ascetic personality components from the full context of the lives and political settings of his leaders in order to concentrate on this one variable alone and its potential meanings. He is explicit about his search for a model abstracted from the surrounding social context: "We have sought to delineate the characteristic traits, the pattern of behavior, that might throw light on our general subject of revolutionary asceticism" (p. 141). Mazlish is aware that all revolutionaries were not ascetic, and that personal asceticism is not limited to revolutionaries. Danton is reported to have defined virtue as what he did with his wife in bed at night (p. 83). Another type of leader, in this case one that fits Mazlish's criterion of asceticism, is the leader of modern nationalist movements such as Mohammed Ali Jinnah, Mohandas Gandhi, Moammar Khadafi, or Theodor Herzl. They were (or are) personally ascetic, and the appeal to their followers included their apparent ability to transcend the needs of the flesh.

There are those, such as Weinstein and Platt, who hold that the only appropriate psychohistorical analysis of revolutionary situations is in the specific ego psychology and adaptation of the leaders to the given social tensions of a revolutionary situation. Others, such as E. V. Wolfenstein, transcend cultures to postulate a "revolutionary personality" entirely grounded in the family psychodynamics of the actors. Mazlish attempts to pursue a middle course. He maintains that while functioning in a specific historical setting, these leaders have responded to their family background in a particular way, and that is in the intense and unresolved struggle with their fathers of the oedipal complex.

A major limitation of Mazlish's model of asceticism is his virtually exclusive reliance on the oedipal dynamics of Freudian libido theory. His analysis would have been enriched by the use of psychoanalytic object relations theory. Both the personal impoverishment in terms of friendship and family ties that Weber, Freud, and now Mazlish point to, and the striking ability of his subjects to act, inviting others to join them in the certainty of success, speaks to a type of relationship to other people and to the world that is certainly pre-genital and cannot adequately be conceptualized in oedipal terms. Lenin, for example, resolutely drove minor points of difference with opponents to a breaking point. There is a distortion that comes of a unifocal emphasis on one aspect—the asceti-

cism—of these leaders. All of the men Mazlish chose to write about had the ability to act decisively on their personal visions in order to transform the world, or at least an important portion of it, in their own image. This, then, is in the realm, not of oedipal conflict, but of ego psychology and adaptation. Their capacity to use their personal fantasies creatively and to mobilize masses of followers in a political cause is a sign of good ability to regress in the service of the ego, to reality test, and to resolve ambivalence in action.

The value of this book lies more in its vignettes of individual leaders, although these are all from secondary sources, than it does in the conceptual power of the typology of the "ascetic revolutionary." A difficulty with Mazlish's formulation is that "ascetic" is given a narrow sensual definition. Those who are in politics seek power. Is the quest for dominance ever truly "ascetic," even if it abnegates libidinal pleasures? This was precisely Freud's point: displacement takes place from the body and interpersonal relations to the realm of ideology, personal fantasy, and charismatic political leadership. Mazlish has contributed the specific revolutionary contexts of a group of these modern leaders within Weber's vision of the great transformation from "innerworldly" to a "worldly" asceticism.

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C. W. CASSINELLI. *Total Revolution: A Comparative Study of Germany under Hitler, the Soviet Union under Stalin, and China under Mao*. (Studies in Comparative Politics, number 10.) Santa Barbara, Calif.: Clio Books, 1976. Pp. 252. Cloth \$19.75, paper \$6.25.

According to C. W. Cassinelli the revolutionary policies of Hitler, Stalin, and Mao Tse-tung emphasized one-man rule and sudden ideological and social change. Thus Hitler, after coming to power, purged the leftist opposition led by Ernst Röhm. His anti-Semitic measures, culminating in the "Final Solution," profoundly altered German society. Numbering them among his enemies, Stalin eliminated Old Bolshevik revolutionaries during the 1930s. Simultaneously, he used many of their ideas in the Five Year Plans that transformed Soviet life. And Mao Tse-tung, through the Long March, the Great Leap Forward, and the Cultural Revolution, brought fundamental ideological and social changes to China.

The author may have stressed the idea of revolutionary social change at the expense of the need for pragmatic solutions to immediate problems. For example, he omits the joint attempt by Albert

Speer and Joseph Goebbels to convince Hitler in 1943 of the importance of a total war effort. The statement (p. 72) that Hitler played Speer off against the SS, an organization concerned more with racial policy than with production, does not really suffice here.

Cassinelli may also have exaggerated the continuity between Lenin and Stalin when he writes that both men hesitated to share power (p. 109). Unlike Stalin, Lenin at least listened to alternative viewpoints before acting on a given policy.

The theme of revolutionary social change is the common denominator for the three leaders in this study. The book is at best an introductory analysis of revolution. Many readers will want to compare it with the excellent biographies of leading Fascist and Communist policymakers now available.

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ROBERT S. WISTRICH. *Revolutionary Jews From Marx to Trotsky*. Foreword by JAMES JOLL. New York: Barnes and Noble, 1976. Pp. 254. \$16.00.

Robert S. Wistrich's study of ten revolutionary Jews (Marx, Lasalle, Bernstein, Luxemburg, Adler, Bauer, Lazare, Blum, Martov, and Trotsky) admirably succeeds in showing that Marxism provided partially assimilated Jews with a substitute for a Jewish identity which they had lost or wished to lose. The psychological dilemmas confronting Marxists of Jewish origin are discussed in illuminating detail. Wistrich also shows how assimilated Jewish intellectuals introduced into revolutionary socialism a settled hostility to the concept of a separate Jewish group identity. This in itself makes the book a worthy companion to Edmund Silberner's *Sozialisten zur Judenfrage*, which covers some of the same individuals discussed by Wistrich. But some readers will be uneasy about the author's eagerness to equate a lack of sympathy for Jewish national identity with anti-Semitism. His discussion of Marx is a case in point.

Marx clearly detested Judaism, and in suggesting that the rejection of his own Jewishness facilitated his development as a Marxist, Wistrich provides a fruitful perspective on the psychology of revolutionaries. Yet his view of Marx's anti-Semitism too readily dismisses considerations that suggest the need for a more nuanced conclusion. Why, for example, should Marx's support of Jewish political emancipation be written off as a merely "tactical" maneuver? Admittedly, Marx showed no interest in a specifically Jewish national identity, but if Jews were to disappear in the process of human emancipation, a similar fate awaited other nationalities.



Wistrich is most successful in his analysis of the dilemma which the anti-Semitism and Jewish nationalism of the late nineteenth century forced upon Jewish Marxists. Dogmatically insistent that anti-Semitism should be combatted by the overthrow of capitalism and all too eager to deny their Jewish origins, assimilated Jews like Luxemburg, Bauer, and Trotsky showed scant sympathy for the distress of East European Jewry. Others, such as Bernstein and Blum, combined ideological flexibility with a willingness to become active spokesmen for Jewish interests, while the little-known but fascinating Bernard Lazare came to recognize the inadequacy of the Marxist position on the Jewish question. Wistrich has written a stimulating book which deserves to be read by all those who are interested in recent European history.

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RICHARD F. STAAR, editor. *Yearbook on International Communist Affairs, 1976*. Stanford, Cal.: Hoover Institution Press. 1976. Pp. xxx, 636. \$25.00.

With this volume covering 1975 the *Yearbook* series has completed its first decade and has reached its eighth year under Richard F. Staar's editorship. It has become an important reference tool that should be known to all historians working on current affairs. Like its predecessors, this volume consists mostly of individual articles on recent developments in the various Communist parties. Some 91 countries are covered. There is also a section on front organizations, along with a few biographical sketches, a list of about 300 recent books, and an index to the more than 1800 names that are mentioned. Staar has provided a broad introductory survey. Many of the more than sixty contributors and six associate editors are colleagues of his at the Hoover Institution.

As far as I have been able to judge, the coverage is generally systematic and reliable. There are full citations to newspapers and other sources used by the contributors. The proper pronunciation of names is aided by extensive retention of diacritical marks, inverted commas, and apostrophes where they are appropriate, as in Arabic, Wade-Giles Chinese, and various East European languages.

Like any potential user, I can find a few statements to challenge, such as the assertion that the Yugoslav authorities have been battling their dissidents with a "repressive vigor" that has been "surpassing other communist-ruled countries with the exception of the Soviet Union" (p. xxi). On the stylistic side there are some ambiguities and infelicities, but they are relatively few. On such major problems as that of how to define "Communist" or how much historical background to

include, probably no one, including the editors and contributors themselves, can be entirely happy with the arbitrary distinctions and boundaries that have had to be drawn. In view of the limits of space and time, they have done remarkably well.

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## ANCIENT

JOHN CHADWICK. *The Mycenaean World*. New York: Cambridge University Press. 1976. Pp. xv, 201. Cloth \$17.95, paper \$6.95.

John Chadwick has succeeded in one of the most difficult of scholarly accomplishments, and has crafted a readable, trustworthy, and illuminating book for the amateur in the field of Linear B studies. *The Mycenaean World* is essentially a social and economic history of the later Bronze Age in Crete and Greece, interpreted through the archives at Knossos and Pylos. Chadwick's long experience and authority in this aspect of the early Greek language, since he first joined with Michael Ventris in the early 1950s in establishing the ramifications of Ventris' decipherment of the syllabary, are evident throughout this substantial popular book. For many scholars such experience might have made it more difficult to avoid the discussion of details distracting to the clear path of the argument. With self-discipline, and considerable modesty about his own role in the development of Linear B studies, Chadwick has been able to select texts and recreate a nearly-lost way of life in an impressive and judicious manner.

The book is not meant for linguists, and although it will be of great value to serious students of Linear B, they will quickly note that there is not even a syllabary in it. There are fine drawings, good photographs of tablets (some plates are printed too dark), and complete explanations of ideograms and systems of weights and measures but, wisely, no attempt to teach early Greek as such. The aim is rather to show us how the tablets, archives, and scribal system worked, what can be expected from the elaborate, baffling records of the principal palace centers of the Aegean, and how such matters as political and economic dominion, land-holding, industry and agriculture, trade and military preparedness may be recreated as a total picture consistent within itself and consistent with the archeological record as well. It is an extremely interesting book, full of new ideas and interpretations, and it shows constant good judgment in such difficult areas as political hegemonies, con-



tributions by disparate ethnic and linguistic elements in the society to the whole which we call "Greek," the balance of the economy, and the sources and the forms of religious cults. The style is clear and fluent, the amount of information conveyed is enormous while its absorption is made painless. In my judgment, Chadwick's *The Mycenaean World* is the only reliable "history" of the period now available.

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RONALD MELLOR. *Thea Romē: The Worship of the Goddess Roma in the Greek World*. (Hypomnemata, number 42). Göttingen: Vandenhoeck and Ruprecht, 1975. Pp. 234. DM 58.

This is an exhaustively meticulous survey of the cult of the goddess Roma in the Greek world. The arrangement of the material is topical, with the volume divided into two parts; the first treats the history of the cult, and the second describes its forms and institutions. The historical section is largely devoted to a region-by-region examination of the rather exiguous and disparate evidence, seeking to place this material in the context of the political history of the various cities and of their relations with Rome. It is prefaced by a brief chapter that attempts in far too few pages to examine the complex problem of the origin of Roma as a religious personification and cult figure. Similarly, a prefatory survey treating the Greek view of Rome introduces the descriptive portion of the work, with its series of chapters devoted to cult and other honors paid to Roma. An epilogue sketches the later history of the cult of Roma in the imperial age, while the volume is concluded by an extremely useful appendix, collecting and quoting the epigraphical evidence for Roma in the Greek world.

The book is poorly organized and surprisingly repetitive for so brief a volume, problems compounded by the absence of an index. Nonetheless this is a monograph of considerable merit, cautious and rigorous in its methodology and combining a detailed knowledge of the epigraphical evidence with a sure grasp of the political history of the period. Its weakness lies in the author's narrow vision of the topic and his fundamental inability to understand the worship of Roma as a religious and social phenomenon. Ronald Mellor pursues Dea Roma with such single-minded determination that he first argues, then assumes that ancient references to statues of the Demos of the Romans indicate likenesses of the goddess Roma. In fact, of course, the numismatic evidence is clear that in the Greek world the Demos of the Romans was represented as a male, in toga (Synnada) or

with cornucopiae like the Genius Populi Romani (Alexandria). The origins and early evolution of the cult of Roma are sought exclusively in the Hellenistic Greek world. The current of its development is represented as flowing entirely from east to west, and Mellor attempts to force the evidence into this preconceived scheme. He fails to realize that, before the introduction of Roma in the Greek world, the divine power of the collective Roman people was worshiped at Rome in the cult of Genius Publicus, a Roman guise for a concept identical to and taking its impetus from the cult of Demos at Athens. Together with the ruler cult, the worship of the city personified represents a religious response to a transformation in social and political values and institutions. Like the earlier hero cults and the later cult of saints, it is rooted in the recognition of divinity in a power capable of rendering benefits to the community of worshippers. Mellor can conceive of the cult of kings and of Roma as a purely political phenomenon. By excluding the religious dimension he robs the subject and his own work of vital depth and perception.

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JENÖ FITZ. *La Pannonie sous Gallien*. (Collection Latomus, volume 148.) Brussels: Latomus, 1976. Pp. 85. 275 fr. B.

Following the devastation of Pannonia by the Roxolani in A.D. 260 Gallienus rebuilt certain military camps and ultimately reorganized the military and civil administration of the province. Jenő Fitz gathers the rather thin epigraphic, numismatic, and archeological evidence to establish the extent of the damage and reconstruction, and to identify the men on whom Gallienus relied. The conclusions are modest, largely of interest to local historians.

In one regard the study must be challenged. In the longest chapter, "Circulation Monétaire," coins from eight sites, struck between A.D. 193 and 305, are divided into six periods. Fitz then comments on the apparent implications for each site of the varying figures; more coins or fewer suggest variation of economic activity, and relative prosperity when compared with other sites. (The approach has been tried before, for example in Pekáry's work in Vindonissa.) Unhappily the text compares relative frequency of the coins per period to Pannonia as a whole; the graphs to which the text refers compare them, without warning, to Pannonia Superior or Inferior. The results are a jumble: on page 30 Fitz writes that Intercisa prospers, for "durant la Période de 238-253 son indice dépasse la moyenne [scil. of Pannonia] 113,33%" while

the adjacent figure shows Intercisa for this period producing substantially fewer coins than "*la moyenne*" [*scil.* of Pannonia Inferior].

This is an accident; the real difficulties are fundamental. No criteria establish significant variation. On page 20 the author says that coins found at Savaria, struck during A.D. 260 to 268, are six and two-thirds percent commoner than for the whole province, indicating "*une position relativement plus favorable de la vie économique de la ville.*" The percentage is trivial, equivalent to only four coins. No weight can be placed on such data.

More important, Fitz ignores coin production, distribution, and circulation. He indicates neither denominations nor mints, just raw totals. Even if his material is coherent, the pattern of coin finds may reveal variation not in local circulation but in original mint production. This is especially the case when several sites return the same results, and his figures for the sites in Pannonia Superior generally follow one distinctive pattern. Again, coins circulate idiosyncratically. Where they ultimately appear has no necessary relation to locus of issue or major zone of circulation. Some military pay and administrative expenditures may have been made locally in fresh coin, but unless the finds are uncirculated, or sealed in an independently datable context, their date of striking cannot be shown to have local significance. The fifty-five coins from Savaria struck A.D. 260–268, said to illustrate a rise in economic activity, do not prove that any coins were introduced to Savaria during that period; they could all have arrived later. Some coins of Gallienus were particularly common, and continued to circulate well into the fourth century. They therefore prove nothing about his activity at these sites. Until we can identify the coins actually circulating at a given moment, we cannot discuss relative monetary activity, nor argue what that means for the life of a community.

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PIER CESARE BORI. *Chiesa Primitiva: L'immagine della comunità delle origini Atti, 2,42–47; 4,32–37—nella storia della chiesa antica.* (Testi e ricerche di Scienze religiose.) Brescia: Paideia Editrice. 1974. Pp. 303.

No title can tell the whole story of a book but the title of this book does not truly reflect what is actually an examination of texts by the Fathers on the subject of the earliest Church which had its "harmonious" and "unanimous" beginnings in Jerusalem under the Apostles. Dividing his materials between East and West and according to centuries (from the third to the fifth), Pier Cesare Bori deals with the view of the virgin Church held

by some of the Fathers. Taking as their point of departure two passages in *Acts* these Fathers discoursed on the creation and characteristics of the subapostolic Church, which was understood as a community of communal holdings and communal love. Simply put, the patristic perception was either backward-looking to pristine goodness or forward-looking to reform of the Church of their own times.

Whereas the source of their religion or faith lay with Christ, the source of their Church lay with the Apostles in Jerusalem down to the Destruction of the Temple. That Church became a model of the institution in which they operated. At once we can perceive how little the Fathers distinguished between the primitive assembly or community of Christians and the universal religious establishment of a church as we also understand the word church. Putting aside palpable and deep divisions among them, the Fathers agreed in acknowledging their participation in a single Church, directly succeeding the Jerusalemite community and supplying the grounds for unity of belief, deed, and thought in what had become an institution coincident with an empire.

The model of the Jerusalemite community encouraged, not to say enjoined, poverty and almsgiving and opened the road to joining the heavenly community of Christians. While the Fathers saw or anticipated human frailties and sinning, they issued summonses to return to the first Church of the past or invitations to renew the Church of the present by remaking the Church along the lines of the oldest plan. As the author points out, more than nostalgia informed the patristic mind. A tenuous sense of history, a reliance on prophetic or revealed history, and a recourse to the Scriptures, which could (and can) yield many truths to many men, converged to make an argument for, and hence a belief in, the real or potential coincidence of a private sect in Jerusalem with a public institution of the whole Roman empire. To realize the past in the present or to recreate it, the Fathers were almost ineluctably led to promote or admire the artificial community of monastics. These many communities naturally have no historical relation to the one subapostolic community.

The author has done a fine job of tracing the development of the "dovish" dialectic which the Fathers bequeathed to those seeking to conserve the character of one institution in succeeding ages of an oft-changing institution that was already far different from the virgin church at the moment when the Fathers wrote. As usual the endeavor of the Fathers to attain the heavenly church seems lacking in flesh and bone; their interest in men stops at their souls. If they had poorer historical judgment than non-Christians, they were not alien

to non-Christian belief which ever tended to keep men in the thrall of the Past.

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## MEDIEVAL

EDGAR B. GRAVES, editor. *A Bibliography of English History to 1485*. New York: Oxford University Press, 1975. Pp. xxiv, 1103. \$52.00.

The last comprehensive bibliography for medieval English history was Charles Gross, *The Sources and Literature of English History from the Earliest Times to about 1485* (1915). The present work, eagerly awaited by all scholars in the field, has been in the planning stages since the 1930s. Edgar Graves has compiled a valuable guide to the published sources, bibliographies, philological and paleographical aids, and major monographs. The latest entries are from December 1969, for the period preceding 1066, and December 1970, for the period from the Conquest to 1485. Some attempt should be made to revise or supplement this work at set intervals.

The organization of this bibliography follows the general guidelines established by Gross, with a few changes for the better. The expansion of scholarship in the fields of economic and social history is reflected in the allocation of individual chapters to agrarian society, urban society, and land tenures and estates. Local history also receives the individual treatment required by the concentration of modern scholarship in that field. Graves has tried to place entries of modern commentaries near the source materials to which they refer, whereas Gross separated all entries broadly into sources and commentaries. *Festschriften* and collected works are given a distinct section with full listings of the contents of each entry, which facilitates thorough cross-referencing. Graves provides an exhaustive index listing authors and detailed subjects. He does not, however, provide an index of titles for the reports and appendixes of the Historical Manuscripts Commission or the Rolls Series, since these already appear in Gross and in Mullins' *Texts and Calendars*. The individual entries often include analysis, and the presence of a useful bibliography in a monograph is always mentioned. No bibliography of this period could list all modern studies; thus, Graves limits his selections to those writings which present "new or controversial interpretations or include modern specialized bibliographies."

The bibliography compiled by Gross was intended for his graduate students at Harvard Uni-

versity. Graves has the same audience in mind, but his bibliography will be useful for any scholar in the field. The work is a major achievement, a timely and necessary resource.

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JOHN HARVEY. *The Black Prince and His Age*. Totowa, N.J.: Rowman and Littlefield, 1976. Pp. 184. \$14.50.

John Harvey is well known as the doyen of historians of medieval English architecture. In the present book he endeavors to relate the glories of fourteenth-century art to the career and era of the Black Prince, who is regarded fondly as the epitome of aristocratic-chivalric values (also regarded fondly). The book is reminiscent in some ways of John Barnie's *War in Medieval English Society* (1974), but is more episodic and less consistently plausible in finding links between the author's topics than was Barnie with his, the social-military values and popularity of the Hundred Years' War and contemporary literature.

There are, as might be expected, many excellent pages and stimulating insights into the artistic culture of fourteenth-century England, although the suggestion (p. 33) that this was the greatest age, not only of English art, but of the entire Western artistic tradition, might be disputed by more people than simply Giorgio Vasari. But integration of this material with the *persona* of the Black Prince is intermittent, and the treatment of the Prince himself is rarely pursued rigorously or systematically beyond generalized commentary, reminiscent of Froissart, about the military highlights of his career. The tone and argument of the book then shifts abruptly in the last chapters to a view of Richard II consciously endeavoring to transcend the limitations of aristocratic-chivalric-military values. Opposition to Richard is dismissed as mere murderous arrogance and gangsterism; Richard's deposition prevented the guaranteed solution of the Irish question, not to mention of all outstanding questions of Anglo-French relations; and Richard's governance, with its unique and far-sighted vision of justice and peace, would have matched perfectly his own superbly sensitive and refined artistic taste. Unfortunately these chapters go well beyond legitimate historical interpretation to something uncomfortably close to special pleading with certain presentist aims.

The writing is engaging and zestful, and the plan of the book intriguing; but it does not provide a full or balanced account of the careers of the Black Prince and of Richard II, or of the con-

nections between political values and artistic achievement in fourteenth-century England.

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FREDERICK BEHREND, editor and translator. *The Letters and Poems of Fulbert of Chartres*. (Oxford Medieval Texts.) New York: Oxford University Press. 1976. Pp. xciii, 297. \$27.50.

The letter collections of the eleventh and twelfth centuries constitute some of the most important sources for the cultural and administrative history of the central middle ages. Of these, the collections of the twelfth century are more extensive and orderly. Their principles have been described in Giles Constable's edition of *The Letters of Peter the Venerable* and in several studies by R. W. Southern. Some of them, particularly that of Heloise and Abelard, are well known beyond a specialist range of scholarship. The collections of the eleventh century are fewer and less well known, but no less important. They illuminate a world with fewer sources and more vexing questions. Frederick Behrend's edition of the letters and non-liturgical poems of Fulbert, bishop of Chartres from 1006 to 1028, is an important and welcome contribution to eleventh-century studies.

Fulbert, perhaps best known to political historians for his famous letter to William V of Aquitaine explaining the oath of vassalage (no. 51 in this collection), and to other historians for his literary and educational career in the school of Chartres and for his devotional works, was an important figure in other ways as well. The bishopric of Chartres took precedence over all others in the province of Sens, and the leading French bishops of the early eleventh century were an influential group of spiritual and temporal lords. Not only remarkable learning and literary activities, but spiritual and political versatility characterized many of the bishops of early eleventh-century Francia. Lacking the spiritual ideology of later reformers and the legal powers and papal support of later prelates, many of them labored heroically at ill-defined and always obstructed administrative and pastoral duties, castigating the shortcomings of their colleagues and lay peers as assiduously as they chastised, scolded, and encouraged their subordinates. They, with their colleagues in the Empire, marked the first major revival of episcopal authority since its Carolingian beginnings.

Behrend's edition of Fulbert's letters contributes much to the illumination of their world. Fulbert, probably a pupil of Gerbert and certainly the first luminary of the school of Chartres, appears in these letters less as schoolman than as bishop. He corresponded with the great and small powers of his world: Abbo of Fleury, Fulk Nerra of Anjou,

King Robert of Francia, Adalbero of Laon, St. Odo of Cluny, and, in a charming, surprised note, Cnut of Denmark and England (no. 37). He also wrote to unnamed scholars, clerics, various lay and clerical vassals of Chartres, and to his fellow bishops. His letters add to a growing body of scholarship on the period, and Behrend's edition takes a proud place in a field recently opened up by the work of J.-F. Lemaignier and R. W. Southern and marked by the editorial labors of R.-H. Bautier and others.

The edition and translation of Fulbert's letters poses unusual problems, even for an eleventh-century source. Following a suggestion of M. Gibson and Southern, Behrend reconstructed the dating sequence and grouping of 131 letters from a group of manuscripts, none of which is authoritative and all of which pose considerable editorial difficulties. Behrend's description of the manuscripts and the principles of dating and grouping the letters (pp. xlii-xc) is in itself a fine exposition of the methodological difficulties of the editor's task. Part 1 of Behrend's introduction incorporates most of the best recent scholarship on Fulbert's life, times, and interests. Part 2 contains Behrend's masterful description of the manuscripts, editorial history, and editorial principles of Fulbert's *corpus*. The texts and translations, in the by now familiar and very convenient Oxford Medieval Texts format, are followed by three appendices, and by indices of manuscripts, quotations and allusions, and *generalia*.

Particularly interesting in these letters is Fulbert's and others' use of canon law, especially the earlier collections of Benedictus Levita and Pseudo-Isidore (see p. xxiv) to deal with the diverse problems that confronted an early eleventh-century bishop. These include abbatial and episcopal elections and appointments (nos. 1, 22, 26, 28, 42, 52-56, 128-130); ecclesiastical discipline and methods of proof (nos. 25, 71, 77, 79, 86); one particularly gruesome murder case (nos. 29-36); and numerous other legal matters ranging from marital problems to the duties of the lower clergy. Here Behrend's notes, both to the manuscripts and to the matters discussed in the letters, are helpful and illuminating. It is always a difficult task to take over an editorial program from someone else and make it one's own, as Behrend has done with the Fulbert material he received from Loren MacKinney. But Behrend has succeeded with great distinction. His edition will undoubtedly remain the standard text for these important documents.

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MAURICE BERTHE. *Le comté de Bigorre: Un milieu rural au bas moyen âge*. (École des Hautes Études en

Sciences Sociales, Centre de Recherches Historiques. Les hommes et la terre, number 15.) Paris: Sevpen. 1976. Pp. 283.

Maurice Berthe's book, a revision of his doctoral thesis presented at the University of Toulouse in 1969, summarizes fiscal data contained in two sets of tax records from the Gascon county of Bigorre: those of 1313 (the *Debita Regi Navarre*) and 1429.

These sources, limited as they are by their nature as tax lists, nevertheless have the advantage of facilitating comparison over the span of a century and of adding to documentation for a region singularly lacking in archival materials. Following a short sketch of Gascon political developments from 1251 to 1429 and definitions of measurements prevalent in the fourteenth and fifteenth centuries, the volume proceeds to an analysis of the 1313 and 1429 records with respect to population trends, political organization, the economy and rural life, patterns of settlement and urban growth, social classes, and seigneurial revenues. The tax lists warrant concluding that the economy was astonishingly stable, remaining one of subsistence polyculture throughout the period despite proliferation of textile mills and a regional demographic decline of around forty percent. Social traits reflected the same stability, with ownership of small farms remaining typical of the peasantry and purchase of vacated lands binding the bourgeoisie more closely to the land even as they accumulated capital through manufacturing and trade. Certain generalizations long accepted by medievalists about Western European economic and social history find corroboration here. Serfdom, characteristic of approximately twenty percent of the rural population in 1313, had declined to three percent a century later. Seigneurial revenues, although they changed very little during this period, fell sharply in real value as a result of currency devaluation.

An idea of the interpretive restraint of the work can be gained from the observation that only one-third of its pages are devoted to narrative text while two-thirds are taken up with accompanying graphic representations of data (tables, maps, graphs) and critical apparatus. One must compliment Berthe on a meticulous piece of research even as one regrets, as he does, the documentary lacunae that prevent the historian from "evoking the actual life of the towns and fields of Bigorre" in the late middle ages.

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PIERRE BONNASSIE. *La Catalogne du milieu du X<sup>e</sup> à la fin du XI<sup>e</sup> siècle: Croissance et mutations d'une société*. In two volumes. (Publications de l'Université de Toulouse-le Mirail. Series A, volumes 23 and 29.) Tou-

louse: Association des Publications de l'Université de Toulouse-le Mirail. 1975, 1976. Pp. 522, 540-1045.

This book is concerned with changes in society in a period when few regions in Western Europe were developing as rapidly as Catalonia; the subject thus has more than local importance. The book's two themes are the economic growth of Catalonia and the simultaneous and interconnected emergence of a greatly enlarged nobility and of a new servile peasantry, the latter persisting in Catalonia until the late fifteenth century. The unusually rich sources (over 15,000 contemporary documents) have scarcely been explored from this point of view. The author does not overstate the possibility of attaining reliable statistics, but his tables of data merit exploration.

To readers of Pierre Bonnassie's earlier articles his book is disappointing. It cannot be compared to the work of P. Toubert on central Italy in this period. Bonnassie's bibliographical equipment is seriously defective. Not only modern Italian and German but much Spanish scholarship is ignored. Nor are Bonnassie's conclusions on social and economic subjects always convincing. The effects of Islamic gold on the economy seem more complex than he states. With regard to Catalán family structures and to the interrelation of agricultural change and urban growth, Bonnassie's work should be compared to the studies of J. E. Ruiz Domenec (American readers can consult "The Urban Origins of Barcelona" in *Speculum*, 52 [1977].) Like Toubert, Bonnassie follows Georges Duby in dating the establishment of feudal institutions later than used to be customary (c. 1020-60, in this case). This conclusion is here based, however, on an a priori definition of "feudalism" as contrary to "public power," a definition which hardly corresponds to the facts of Catalán vassalage. Bonnassie himself notes the use of "*terras de feo*" for institutions often defined as "feudal" in the tenth century (p. 556).

The author's incursions into cultural and religious history are unfortunate. He minimizes the role of monasteries in repopulation (pp. 102-6), fails to see any real difference between Catalonia before 1000 and the rest of Christian Spain, and views the Truce of God as the "spontaneous" generation of "exploited masses" (pp. 636-60). The introduction of the Benedictine Rule (and the Roman Liturgy) into Catalonia in the ninth century, two centuries before the rest of Spain, is ignored. (See Antonio Linage Conde's *Los orígenes del monacato benedictino en la Península Ibérica*.) Bonnassie's tables show the small role played by Catalán devotion to Santiago de Compostela in 950-1050, compared to Italian sanctuaries (see p. 333). The Truce of God, like the earlier Peace, was the work



of churchmen and aristocrats; see H. E. J. Cowdrey, in *Past and Present*, 46 (1970).

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J. N. HILLGARTH. *The Spanish Kingdoms, 1250-1516. Volume 1, 1250-1410: Precarious Balance*. New York: Oxford University Press. 1976. Pp. xx, 455. \$23.00.

This is an important and much-needed interpretative synthesis by a prominent Hispanist (who recently gave us *Ramon Lull and Lullism in Fourteenth-Century France* [Oxford, 1970]). Starting where the Reconquista ends, J. N. Hillgarth compares and (especially) contrasts Castile with Arago-Catalonia: Spain of the high plateau with Mediterranean Spain. Much of this first volume details the economic-social, institutional, and religious data (the Church and "the three religions" receiving much attention); the remainder is devoted to the political history of each country (1250-1325) and their "struggle for hegemony" (1325-1410). Vignettes and source-translations catch the flavor of each society. Granada, Navarre, and Portugal play peripheral roles. Hillgarth's style is easy and clear, his grasp of current and past bibliography unusual. And his positions are forthright—he is cautious about some of Jaime Vicens Vives' economic history and partial to Américo Castro's main theses; he dismisses J. Lee Shneidman's *Rise of the Aragonese-Catalan Empire, 1200-1350* (1970). His absorbing detail, culled from recent scholarship and from figures like Lull and Juan Manuel, marvelously illumines society and sensibilities.

Any survey involves selection and limitations; any chronology affects texture. Negative criticisms therefore tend to reflect the reviewer's own preferences or stance. Hillgarth leans toward the fourteenth century—Lewis' "Closing of the Medieval Frontier," and the budding Renaissance—with the thirteenth treated oddly as prologue and kin. The chapter on the Church is institutional and, within that sphere, preoccupied with moral failings, thus reflecting the passionately reformist sources with less attention to their redeeming religious context. One major theme (elaborated also in Hillgarth's 1975 supplement to the *English Historical Review*) is his rejection of the popular notion of Arago-Catalan "imperialism" and a corresponding assertion of Catalonia's military and economic weakness. Hillgarth's healthy corrective tips too far in the other direction and rests on shaky premises. Its terms are not equal in comparing resources of an inland patriarchal king with those of a city-state region presided over by a count-king, for example; the exuberant expansion

of Catalan trade and presence, with all their implications, was not diminished by the political incapacities of a count-king. Further, military power cannot be stated solely in terms of galley or knight numbers. It is becoming clearer too that the Jews did not play the overwhelming financing role traditionally assigned to them; in fact, Catalan crown finances are a *terra incognita* only now being explored. It is a tribute to the quality and force of Hillgarth's work that it rises above detail and invites such debate.

The product of a distinguished career in Arago-Catalan research and teaching, *Kingdoms* immediately becomes a standard tool for medievalists and Hispanists. With Joseph O'Callaghan's *History of Medieval Spain* (1975) it fills a half-century lacuna and will be widely welcomed.

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Los Angeles*

*Acta Germanica*. Volume 1. *Statuta Nationis Germanicae Universitatis Bononiae (1292-1750)*. Introduction by PAOLO COLLIVA. Bologna: Associazione Italo-Tedesca. 1975. Pp. 316.

The great legal *studium* of Bologna was a complex of corporations, of which the German Nation of students was among the strongest. Its ten known sets of statutes, scattered from 1292 to 1798, are here edited in a workmanlike way, almost all for the first time. The preface and historical introduction are given in both Italian and German; notes and index are sufficient and lucid.

The young lawyers who drafted the statutes had to maintain the safety, honor, and interests of their fellow German-speaking students by providing for their governance, corporate solidarity, common purse, and civic reputation. The election and duties of officers function in the statutes as the basic political structure of the Nation: two proctors, two councillors to the rector, two scrutators or syndics, all as early as 1345, later a librarian (1598) and beadle (1610). Solidarity was insured from the beginning by prime jurisdiction, common worship, funerals, convocations; no member of the Nation could enter a partnership outside it. The common chest, in the proctors' quarters (1345) or in the Nation's select church of Santo Fridiano (1497), was filled by initiation dues (first a good-faith tax on income, with deductions for living expenses, later a lump-sum payment graded by class), and by fines for absence at worship and convocation. The proctors were to recruit any subject of the Empire, even non-German-speakers, and to offer honorary membership to notable visitors; the only exclusion applied to the "mechani-



cal" class (1574). Members were expelled for perjury and, to avoid corporate reprisal, bad debts.

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HEINRICH APPELT, edited with the assistance of RAINER MARIA HERKENRATH *et al.* *Die Urkunden der deutschen Könige und Kaiser*. Volume 10, part 1, *Die Urkunden Friedrichs I., 1152–1158*. (Monumenta Germaniae Historica.) Hanover: Hahnsche Buchhandlung. 1975. Pp. xiv, 566.

The book under review is part of the *Diplomata* series of the *Monumenta Germaniae Historica*, which began in 1879. It contains the annotated texts of the available charters of Emperor Frederick I Barbarossa from the first years of his reign (1152–1158). Because of his long reign (to 1190) and the immense output of his chancery, the editors have decided to bring out volume 10 in four parts. The first part does not contain a bibliography, which will follow in part 4. However, almost two hundred pages of indices of legal terminology, names, places, and things are included. These are extremely thorough and will be an invaluable aid to discovering names of witnesses and other participants in the granting of each charter as well as to helping our understanding of the formulized language used by the imperial chancery.

There are 219 documents in this book. Of these, 94 were taken from the original charters extant in various archives. (Unfortunately, five of these were destroyed in 1943 during the war.) Seven documents are forgeries, usually as old and almost as interesting as the originals; only one was done later than the medieval period. The remaining 118 are copies of charters found in various cartularies and other copy books; the best are those contained in the *Codex Wibaldi*, a collection of the writings of Wibald von Stablo Abbot of Corvey (in Saxony), who was an imperial advisor during Conrad III's reign and also in the early years of Frederick. Most of the charters are the records of gifts to organizations and individuals or the confirmation of such gifts made by previous rulers. Several of the documents, however, are major political statements such as DF 1.5, a letter to Pope Eugene III announcing Frederick's elevation to the throne; DF 1.151, a copy of the *Privilegium* for the creation of a separate Austria; and DF 1.186, the letter relating to the Besançon incident and the resulting quarrel with Pope Hadrian IV.

Each charter entry is headed by the date, place of issue, and the state of the manuscripts available. Then follows some discussion of the administrator who dictated the content and of the various chancery clerks who recorded these instructions in

the special chancery style. This gives the reader some insight into the inner workings of the imperial bureaucracy and the decision-making process which involved the emperor and his various advisors. The text of the charter (in Latin and brilliantly edited) concludes each entry.

Judging from my own experience of working with the earlier volumes of the *Diplomata* series, I think the labor of Appelt and his many able assistants will be much appreciated by scholars everywhere. It is to be hoped that the other parts of volume 10 will follow soon, and that similar volumes on Henry VI and his successors will appear in the not too distant future.

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LOTHAR DRALE. *Der Staat des Deutschen Ordens in Preussen nach dem II. Thorner Frieden: Untersuchungen zur ökonomischen und ständepolitischen Geschichte Altpreussens zwischen 1466 und 1497*. (Frankfurter Historische Abhandlungen, number 9.) Wiesbaden: Franz Steiner Verlag. 1975. Pp. viii, 200.

MARIE-LUISE FAVREAU. *Studien zur Frühgeschichte des Deutschen Ordens*. (Kieler Historische Studien, number 21.) Stuttgart: Ernst Klett Verlag. 1974. Pp. 186.

In their fascination with the Teutonic Knights historians have produced a vast and complex literature on the subject. In very different ways these two works add to our knowledge. Marie-Luise Favreau's *Studien*, completed as a dissertation under Hans Eberhard Mayer and Karl Jordan, is concerned solely with the problem of the relationship between the German Hospital in Jerusalem, which fell with the city in 1187, and the Teutonic Knights, who came fully into being in 1198. As Favreau indicates at every turn, her conclusions, based solidly on the sources and painstakingly argued, clash with many of those of the established authorities.

Although the German Hospital was subjected to the Hospitalers by Pope Celestine II in 1143, it retained its own archives and property. Through it the Hospitalers evidently intended to establish a foothold in Germany and hence had every reason to assert the right of inheritance after 1187. Curiously, however, they did not argue this on the basis of the 1143 privilege, but instead on the grounds of an undoubtedly forged papal charter granting the order a monopoly over hospitals in Acre. Here the Hospitalers had clashed earlier with the German hospital founded in 1189–90 and ambitiously hoped through this claim to acquire both the legacy of the hospital at Jerusalem as well as control

over its putative successor in Acre, even though there exists no demonstrable continuity between the two. The whole effort backfired, for out of the hospital at Acre developed, in the later 1190s, the Teutonic Knights, who then between 1208 and 1220 under Herman of Salza established *de facto* control over the Jerusalem hospital, which Frederick II legally recognized in 1229. The Hospitalers abandoned their case only in 1258. One wonders what might have happened if they had built it instead on the charter of 1143.

Lothar Dralle's intention is to examine closely the Prussian lands of the Teutonic Knights between the conclusion of the exhausting Thirteen Years War in 1466 and the election of the first high-born Grand Master, Duke Frederick of Saxony, in 1498, a period which Dralle believes has been particularly neglected in the history of the order. The longest account is that of Johannes Voigt (nearly 250 pages), but it dates from 1839, is predominantly narrative, and says virtually nothing about the relations between the ruler and his estates. Although Dralle's account does discuss the estates and is far more analytical than Voigt's, it is open to question whether it completely fills the bill. Dralle's conclusions, which the reader is left to garner for himself, are not really new. There is, moreover, a tendency toward overabstraction and verbosity; one searches in vain for the crispness of presentation found, for example, in F. L. Carsten's *Origins of Prussia*. The merits of the work lie principally in its richness of detail, particularly of the numerical sort, which does help to fill out our knowledge. Social and urban historians, for instance, will find the chapter on the cities useful (pp. 54–80). In the end, however, the work remains an unsatisfactory mixture of narrative and analysis, generalization and detail, sadly lacking a conclusion which might have pulled the whole work together.

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JOHN WRIGHT, translator. *The Life of Cola di Rienzo*. Toronto: Pontifical Institute of Mediaeval Studies. 1975. Pp. 166. \$5.00.

John Wright must be commended for a readable translation of the anonymous *Life of Cola di Rienzo* from the fourteenth century Romanesco, selected as the chronicler himself says "so that it may be useful to everyone who can read simple texts, such as common merchants, and many other excellent people, who are not students of literature" (p. 23). Wright's introduction contains a superb synopsis of the political and intellectual motives behind the short career of Cola: the ideal of Rome as the *caput mundi*; the ideal of the communal government of the *popolo grasso* and minor nobility as opposed to

baronial rule; the theme of Italian unity as part of a grand imperial design; and the arrival of the age of the Holy Ghost from the mystical beliefs of the Fraticelli.

Written in the mid-*trecento* by an obvious member of the bourgeoisie and consisting of four chapters from his larger *Historiae romanae fragmenta*, the *Life* adheres to the medieval chronicle tradition, adding nothing to the development of Renaissance historiography, except perhaps the lack of a theocentric explanation of events. It reflects the influence of an earlier revival of Livy and city-state history in the Padua of Albertino Mussato, although no attempt was made to model the classical style. The chronicler, like Cola himself, has accepted Orosius' division of world history into four monarchies, with the last, Rome, yet enduring. The only welcome additions to a text which still remains the indispensable source for Cola's biographer would be a more copious use of Cola's letters in the footnotes and a map marked with the principal places of Cola's Rome.

The *Life* affords many succinct insights into the character of Cola. "He was a man like any other; he was afraid to die" (p. 150). Thus the chronicler epigrammatically explained the cowardly behavior of Cola moments before his death in 1354. Weakness was fatal in the face of a hostile mob. Cola's essential humanity emerges. A man from lowly origins with delusions of grandeur, even claiming to be the bastard of emperor Henry VII, he aspired to riches, fame, and power. Dubbed a knight of the Holy Ghost with pomp and ceremony at the Lateran, the Tribune audaciously bathed in the basin of Constantine where, according to tradition, Pope Sylvester had cured the emperor from leprosy. When Cola's young and beautiful wife went to St. Peter's, "serving maids with subtle little feathers made a breeze before her face, and industriously fanned her, so that her face would not be offended by flies" (p. 63). Aping noble ways proved disastrous for the defender of the downtrodden against rapacious barons. Stephano Colonna so warned him in taking the hem of Cola's scarlet robe trimmed with fur, "For you, Tribune, it would be more commendable to wear the plain clothes of a poor man than these pompous garments" (p. 75). The chronicler well delineated the fall of Cola; he left his rise to power less well explained.

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JOHN V. A. FINE, JR. *The Bosnian Church: A New Interpretation. A Study of the Bosnian Church and Its Place in State and Society from the 13th to the 15th Centuries*. (East European Monographs, number 10.) Boulder, Colo.: East European Quarterly; dis-

tributed by Columbia University Press, New York. 1975. Pp. ix, 447. \$17.50.

John V. A. Fine, Jr.'s book on the medieval Bosnian Church is an extremely lucid study of a confusing historical phenomenon. The Bosnian Church has generally been viewed as a "Manichean" religious institution with close spiritual ties to the dualist Bogomils of Bulgaria and Cathars of northern Italy. The sources for this interpretation, however, are largely Italian, and thus at least one step removed from the facts they provide. On the other hand, Slavic sources (particularly from Catholic Dubrovnik, which had close ties with Bosnian churchmen) present a very different picture of the Bosnian Church, a picture that coincides with the few Bosnian sources extant. It is the picture which emerges from the Slavic sources that Fine accepts: the Bosnian Church was a hierarchically arranged monastic community, quite orthodox in doctrine but independent of both the Roman Catholic and Eastern Orthodox Churches, which filled the vacuum in the church life of Bosnia from the thirteenth century until Bosnia became a mission field for the Franciscan friars in the mid-fourteenth century. Dualist tendencies probably did exist in Bosnia, but essentially outside the main body of the Bosnian Church. The misconception that the Bosnian Church was "Manichean" probably stemmed from some minor dualist influence on the church, if not from simple confusion.

Fine argues convincingly that such an interpretation is the only one which fits with the otherwise contradictory sources. His careful analysis and weighing of the sources, their contexts, and their prejudices, suggest that he is right. Fine's description of the Bosnian Church as a few related monastic communities, possibly of the Slavic Latin rite (an aspect to which Fine should perhaps devote more attention), serving as a "nativistic" reaction to Hungarian Catholic threats helps explain why the Bosnian Church disappeared so rapidly when the Franciscan missionaries and Serbian Orthodox clergy began to move into Bosnia and neighboring Herzegovina. Essentially a monastic institution, the Bosnian Church, unlike the Catholic and Orthodox Churches with their parish-oriented structures, could not play an effective role in the daily life of the laity and thus had little popular support. Fine's elaboration of his thesis within the context of Bosnian history and, particularly, against the background of the anthropology and folk culture of the region, makes this study an important addition to our knowledge of the medieval Balkans.

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H. R. ELLIS DAVIDSON. *The Viking Road to Byzantium*. London: George Allen and Unwin; distributed by Rowman and Littlefield, Totowa, N.J. 1976. Pp. 341. \$20.00.

For the casual reader this book is a delight, for the serious historian it is light—at best a fringe benefit. Drawing upon Scandinavian folklore, mythology, runeology, sagas, eddas, etc., and some of the historical sources from Russia and Byzantium, the author produced a sort of Davidson's *Golden Bough*, a melange of good stories and interesting speculations, but not history. H. R. Ellis Davidson is well aware "that the hard core of fact behind the stories is not easy to establish" (p. 207) and studs her narrative with monitory phrases even as she leads us cheerfully through the twilight zone of her subject matter.

We can take it as a hard fact that the Vikings of the ninth, tenth, and eleventh centuries "got around." The author presents a mass of evidence, never really in dispute, to the general effect that they traveled east and south as well as west and south. The testimony of the Swedish runestones, the Icelandic sagas, the coin hoards, the Byzantine historians, etc., is conclusive. When it becomes a matter of identifying the different adventurers and their varied adventures, sorting out where "they" went, whom they served or fought, what they saw, where they died, or what they brought back, however, nothing is very conclusive. Davidson does not successfully sort out the goodies in her grab bag. Indeed, she may add to the confusion as she throws in additional clues, hints, and guesses rather than disposing of any with finality. A certain carelessness in the editing—some incorrect dates, for example—also burdens us unnecessarily. The perennial controversies about Scandinavians in the East, meanwhile, are not moved a centimeter toward resolution, nor is the influence of Eastern art, legends, and customs, upon Scandinavian culture established. These matters are discussed, the various possible connections are suggested, much of the pertinent literature is referred to, and all this makes the book good reading. Given the fragmentary or unreliable nature of the source material perhaps that is all we should expect.

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## MODERN EUROPE

URS BITTERLI. *Die "Wilden" und die "Zivilisierten": Grundzüge einer Geistes- und Kulturgeschichte der europäisch-überseeischen Begegnung.* (Beck'sche Sonderausgaben.) Munich: Verlag C. H. Beck. 1976. Pp. 494. DM 35.

In the last decade there has been increased interest in the study of accounts written by the explorers, merchants, and travelers who discovered new lands and peoples during the age of European reconnaissance and discovery from the fifteenth to the eighteenth centuries. Urs Bitterli believes that European colonial history is not just a story of power politics and economic exploitation but is also the history of the coming together and mixing of a variety of peoples, cultures, and life styles. The cultural interaction of Europeans with natives of Africa, the New World, and the islands of the Pacific had a dramatic impact not only on non-Europeans, but upon the literary history of the European Enlightenment.

Rather than introducing new material to the reader, the author presents an encyclopedia of early modern European travel history. He adroitly summarizes much well-known information. As a case in point, he describes the initial encounters of Europeans and natives, meetings which were characterized by ignorance on both sides. Soon, the superiority of European technology and military power became apparent. What was soon to become a collision of cultures was marked by wars and trade, slave hunters and missionaries. Bitterli's work is wide in scope and interdisciplinary in method. It is not uncommon to encounter a section on the reaction of Voltaire to non-Western cultures followed in short order by a description of the everyday life of the black slave on the "*Zucker-farm*" in the West Indies.

One result of the creation of a large body of travel literature describing non-Western cultures and peoples was the development within Europe of a strong curiosity about the exotic. Soon non-Western natives began to visit England, France, and other Western countries; the native visitor became an object of interest and of study to scholars. As a result we have the beginning of racial classification, the beginning of early anthropology, and ultimately an enhancement of European ethnocentrism.

In the second major portion of this volume, the author portrays the cultural vitality of Europe up to the beginning of the French Revolution. He outlines various European attempts to understand nature and to explore the horizons of knowledge; he examines the work of naturalists, the rise of learned societies, the work of French Encyclopedists (Diderot and d'Alembert), and the possibilities of cross-cultural dialogue in the writings of Montesquieu, Voltaire, and Rousseau.

After a very long introduction, Bitterli examines a crucial problem, the nature of the travel books that generated and helped to form European attitudes toward the non-West. The author describes well the critical and editorial efforts of Richard Hakluyt and the work of Dutch printers. As for

France, the author writes (p. 248) that travel literature appeared later there than in other countries. One wonders if the author is as familiar with the travel literature itself as he is with its supposed importance. He seems unaware of the fact that travel literature printed in the French language (presumably for a French market) was published in German cities such as Marburg, for example, the work of Han Staden (a Hamburg merchant who traveled in Brazil), *Véritable habite par des hommes Sauvages* (1557). Similarly the famous *Cosmography* of Sebastian Münster was published in a French edition (1568) relatively soon after its appearance in German (1544). This criticism is not serious since Bitterli's omissions are not numerous, but the sequence of publication and translation is important in estimating the impact of travel books on European culture. Bitterli's volume is without question a contribution to the general field of travel literature and the cultural significance of the age of reconnaissance and discovery for European literature and patterns of intellectual discussion and debate. Although the book is too general to be of great value to the advanced student, it is an essential tool for the novice. The study is somewhat dated; the author appears to be unaware of important interpretative articles on the nature of travel literature published in major U.S. journals. Further, the author essentially ignores the manner in which the travel books were published and edited in the early part of the age of discovery and reconnaissance. He ignores some important factors in assessing the impact of travel literature upon the intellectual history of early modern Europe.

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JAMES ALLEN VANN. *The Swabian Kreis: Institutional Growth in the Holy Roman Empire, 1648-1715*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 53.) Brussels: Les Éditions de la Librairie Encyclopédique. 1975. Pp. 338.

James A. Vann has written a detailed study of the Swabian "Circle" during a period in which corporate provincial governments experienced a brief flourish as centers of German political life. Originally established in the sixteenth century to look after details in the raising of Imperial armies, the Circles in the Southwest rose to importance when the political paralysis of the Imperial Diet forced them to take the chief responsibility for fending off the French on one frontier and the Turks on the other. Within the Swabian Circle, the larger estates were temporarily willing to cooperate with the smaller estates because of the need for internal consolidation and because of the threat of con-

quest by Louis XIV. The Kreistag, the assembly of the Circle, unified the diverse estates by protecting property in privilege as well as in things. The meetings of this assembly were occasions for baroque protocol, but protocol which established relationships for the hard business of political bargaining. Vann gives the disputes over status and precedence, often dismissed as unimportant by modern scholars, the serious attention they deserve. The Kreis not only cared for the raising and quartering of troops, but it also executed a complex monetary policy and worked to prevent disturbances in the existing order of independent estates. The Habsburg authorities played a self-contradictory role toward the Circle: often it was not clear whether they were more serious about meeting the needs of the Empire or fulfilling the special territorial and religious goals of the Habsburg dynasty. After the end of the War of the Spanish Succession, the desire of the large estates to resume independent policies of expansion doomed the Swabian Circle to decay as an institution.

This book argues that the provincial corporate governments under the Reich provided many of the services of a full-scale government, and that the assemblies of churchmen, princes, and towns proceeded rationally to meet perceived problems. The wreckers of this order of property and privilege were not the smaller estates but rather such large territorial states as Württemberg, or even Habsburg Austria.

This balanced, readable book combines a detailed analysis of a premodern political institution with a warm appreciation of the German society which produced it. Now the latest and best study of German provincial corporations is available in English.

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JACQUES SOLÉ. *L'amour en Occident à l'époque moderne. (L'Aventure humaine.)* Paris: Albin Michel, 1976. Pp. 311. 39 fr.

In spite of the title, this book is not so much a study of love as of sex. It is one of a series of volumes published under the general title of "L'Aventure humaine" under the general editorship of Robert Delort. Basically Jacques Solé examines attitudes toward sex and sexual practices in the period from 1500 to 1800 with emphasis on France, but with considerable coverage of England, the Netherlands, Belgium, Germany, and with occasional glances at Spain, Italy, Denmark, and the American colonies, as well as brief references to other areas. The author attempts to be all-

encompassing. Since Solé's doctoral dissertation was on seventeenth-century French confessional literature, it is understandable that the section on religious attitudes is particularly good. He also provides good brief summaries of the legal and medical literature. Almost no aspect of sexual behavior is left untouched. Included are discussions of prenuptial relations, marital coitus, illegitimacy, masturbation, celibacy, homosexuality, bestiality, sadism, adultery, prostitution, nudism, eroticism, pornography, clothing, and attitudes toward the female. In the process of his investigation the author challenges many of the assumptions about illegitimacy recently popularized by some writers.

A major criticism of the study is the lack of footnotes, a lack which makes it difficult to determine where the author acquired his information or how valid it might be. In general I agree with most of his summaries but some seemed to be based on doubtful interpretations which I could not easily check. The book also lacks an index. In a book designed for the general reader these might not be serious flaws, but the general reader, even if the book was translated into English, would have great difficulty. Solé assumes the reader will have a scholar's grasp of early modern history. Individuals and movements are not identified or explained. Most Americans would know Shakespeare, and probably individuals such as Montaigne or Rabelais; others are known only to the specialist. Despite these deficiencies, the book is still highly recommended. It has material that will enliven any lecture. Solé knows his field and he avoids most of the facile generalizations made by many who have begun to write in this area.

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JOHN P. MCKAY. *Tramways and Trolleys: The Rise of Urban Mass Transport in Europe.* Princeton: Princeton University Press, 1976. Pp. x, 266. \$14.50.

Before streetcars and automobiles, most urban-dwellers lived in concentrated central areas close to sources of work. Frequently, the residence was the place of work. John P. McKay has written a useful and interesting book about one important aspect of the transport revolution that helped change this pattern: the transition from horsecars to electric trolleys. His focus is on Europe, but he traces American influences and makes some European-American comparisons.

The initiative for change from horsecars to electric trolleys came from a few private electrical manufacturers, who crossed national boundaries. As McKay says, "What American enterprise was



to France and Great Britain, German and French enterprise was to the countries of the Mediterranean. And what Belgian entrepreneurs were to Eastern Europe and the Middle East, their British counterparts were to the colonies and Latin America" (p. 244). The technology was often conceived in Europe but developed first in the United States. Although the U.S. was far ahead of Europe in adopting electric trolleys, after 1890 Europe soon caught up and took the lead. One reason for delay was that European municipalities were much more concerned about urban beauty and welfare for the poorer classes. American cities gave long-term franchises to private street railroad companies; Great Britain leaned more to municipal ownership. American municipalities exercised little influence over the companies' internal financial arrangements and in general relied on competition in the free marketplace as a regulator; Europe recognized sooner and more generally that public transport was inherently monopolistic and should be closely regulated. McKay concludes that Americans should have been and should now be more careful to assess the long-range impact of new technology.

Probably because of the book's heavy reliance on official reports, the tone—while pleasant—is somewhat antiseptic and technocratic. Corruption and colorful personalities are barely mentioned; we see little of the heat and dust of connivance and conflict. We are given clear narrative description, with relatively little application of rigorous social science methodologies or explicit linkages to relevant social science theories. The author concentrated on getting those trolleys invented and installed. Consequently, as he points out, there are numerous lacunae, especially in analysis of effects. His discussion stops at about 1910. Among many subjects not fully considered is the impact of the transport revolution on family life and the situation of women. More detailed crosscultural city-by-city general impact comparisons are needed.

The author relied mainly on French, German, and British sources, and has not surveyed the whole European situation. He does not mention, for example, several books about trolleys published in the Netherlands in the 1960s. But his book is a respectable beginning. He and other scholars should go on from here.

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FERNANDO CLAUDIN *et al.* *Problemi di storia dell'Internazionale Comunista (1919-1939): Relazione tenuta al Seminario di studi organizzato dalla Fondazione Luigi Einaudi (Torino, aprile 1972)*. Edited by ALDO AGOSTI. (Fondazione Luigi Einaudi, "Studi," number 16.) Turin: the Fondazione. 1974. Pp. 253. L. 3,800.

This collection of seven essays is the product of a seminar held at the Einaudi Institute in the spring of 1972. The editor had previously written an important study on the discussions concerning fascism at the Fourth Congress of the Comintern. He is now engaged in publishing a lengthy documentary history of the Communist International.

In his excellent introduction, which contains useful bibliographic notes, Aldo Agosti declares that some of the contributions appear in their "original conversational form," whereas others "have been substantially rewritten." We are therefore forewarned that this, like nearly all such collections, will be uneven in quality. Nearly all of the contributors are on the left, but that term covers a lot of ground in Latin Europe! The editor, Paolo Spriano, and Ernesto Ragionieri (d. 1975) are active members of the PCI; Fernando Claudin, Robert Paris, and Massimo L. Salvadori are in the non-Communist Marxist Left; Annie Kriegel and Leo Valiana are now apparently without any specific political commitments.

Chronologically, the book is divided in the conventional manner: Kriegel and Salvadori write on the *biennio rosso* of 1919-20; Spriano and Ragionieri deal with problems of the United Front period and its immediate aftermath; Paris' subject is the third period of "class against class"; and Valiani and Claudin have essays on the politics of the Popular Front.

Each contribution adds something to our understanding of the history of the Communist International, but limitations of space permit mentioning only a few. Kriegel suggestively views the early history of the Comintern as an internal struggle between the "extreme left" ("a kind of orthodoxy or revolutionary centrality") and the "ultra-left," a mixture of Marxism and anarchism which usually, but not always, took the form of "Sovietism" (p. 34). Salvadori is convincing in his demonstration that no country in Europe was "objectively" revolutionary in 1919-20, except Russia.

The late Ernesto Ragionieri's essay on the program of the Communist International is the longest and probably the most important contribution (it was previously published in *Studi Storici*, 1971-72). Ragionieri proves, at least to the satisfaction of this reader, that the debate preceding the adoption of the program of the Sixth Congress of the C.I. in 1928 was remarkably open. It was, he states, "perhaps the last great public debate . . . in which the several contrasting theses on the roads to world revolution openly clashed" (p. 107). The contrast between this debate and much of the remaining proceedings of the Sixth Congress, best known for its initiation of the notorious "Third Period," is itself remarkable. Ragionieri devotes several pages to Clara Zetkin's speech on the program. Her contribution was indeed on a very high



level. In contrast to the position of "class against class" adopted by the Congress, Zetkin urged the importance of winning over the "petty and middle bourgeoisie of the cities and within them the important strata of the intellectuals." She criticized the program's rigid and harsh attack on religion, and her critique of the program's section on the woman question is positively inspired and of great contemporary relevance.

Paris' essay is notable both for its extremist, almost bizarre, conclusions (for example, the German Communist Party was pushed to its "suicide" in order to avoid the "birth of competitive experiences" which would detract from or affect the Soviet experience [p. 191]) and for many acute observations. Among these are his reconstruction of the prehistory, from as far back as 1921, of the "class against class" tactic. Another is his interesting, and at least partially valid, point that in the Third Period the industrial proletariat "plays less and less of a role in the tactics—as well as the strategy—of the communist parties." The practice of "class against class" is based essentially on "the marginal sectors of the world of capitalist production—'oppressed nationalities,' 'subject peoples of the colonies,' [and the] 'totally and partially unemployed' " (p. 177). Paris notes that it is certainly not by pure chance that some of the classical documents of the Third Period have been published by the French Maoist organizations.

This highly readable and provocative book is a useful addition to the literature on the Communist International.

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PAUL GORDON LAUREN. *Diplomats and Bureaucrats: The First Institutional Responses to Twentieth-Century Diplomacy in France and Germany*. (Hoover Institution Publications, number 153.) Stanford, Calif.: Hoover Institution Press, Stanford University, 1976. Pp. xviii, 294. \$11.00.

International affairs are generally approached from one of two directions. Diplomatic historians usually focus on the specifics of "high policy"—policy formulation, personalities, and crises—while political scientists seek patterns and typologies in events, institutions, and individuals. Paul Gordon Lauren's book successfully combines both methods by analyzing reforms instituted in the French and German foreign ministries after World War I in comparative and national terms.

Political, social, and intellectual developments at the beginning of the twentieth century produced mounting pressure from the French and German public, parliament, and press for "modernization" of their traditional, small, and largely aristocratic

foreign ministries in the interest of efficiency, control, and responsiveness to new problems. Organizational structure was most affected: previously separate political and commercial areas were unified, geographic divisions and functional specialties created, administrative and managerial techniques and technological inventions utilized, and propaganda activities augmented. The results were greater size, expenditure, centralization, intricacy, and standardization—all the hallmarks of modern bureaucracy. Professionalization of personnel was sought through uniform and demanding standards of recruitment, examination, training, compensation, promotion and discipline. Considering the institutional resistance, Lauren views these reforms as "impressive" (p. 117) and "remarkable" (p. 153).

He draws a number of insightful conclusions. The experiences of the French and German foreign ministries display the elements of institutional evolution, namely, pressures for reform, personalities of successful reformers, resistance of old personnel and practices, sources of institutional tension, and eventual compromise between traditional procedures and visionary programs. They likewise point up the bureaucratic dilemma of reconciling efficiency and complexity, centralization and compartmentalization, flexibility and standardization, creativity and conformity, and political views and obedience. Lauren contends that these institutions are integral parts of their societies and of contemporary European civilization and that the reforms reflect the transition from "old" to "new" diplomacy, that is, politicization of foreign policy. He implies that "modernization" reduces national distinctions since the reforms responded to general rather than specifically national impulses and were similar in both countries.

At the risk of demanding too much, it is important to indicate what the book does not do. Although a convincing case is made for the similarities between these reforms, the differences are not clear. But if institutions reflect their societies (as Lauren claims) and if French and German societies differed, institutional reforms should have reflected these differences; if the reforms were identical, institutions do not represent their societies. Furthermore, Lauren's claim for the institutional importance of the reforms is not demonstrated. The actual results of personnel reforms are not indicated; other studies of bureaucracy suggest that personnel changes are minimal, even in revolutions. The net result of organizational reforms remains unclear: standardization, centralization, and comprehensiveness were offset by complex new problems, threats of "outside" encroachment, and bureaucratic inertia. Above all, Lauren does not prove the diplomatic historical significance of the reforms, namely, their effect on international

events. Did augmented and modernized foreign ministries exert greater influence? The tendency of interwar leaders to resort to "summitry" and to disregard their diplomats suggests the opposite. Did reformed foreign ministries produce diplomatic departures? The continuity of interwar and prewar policies implies that they did not. Was the "new" diplomacy a departure in content or merely in style, old wine in new bottles? In short, the validity of diplomatic institutional history depends on its influence on international relations.

Lauren has nonetheless increased our understanding of a neglected area. Clearly written, well organized, and exhaustively researched, the book is not only informative in its own right but also a model for this type of study.

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D. E. HOAK. *The King's Council in the Reign of Edward VI*. New York: Cambridge University Press. 1976. Pp. x, 374. \$29.50.

In this exhaustive study of the Edwardian Privy Council D. E. Hoak greatly enlarges our knowledge of government and politics during the reign of Edward VI. Hoak's account of the Edwardian council as an institution—its membership, business, and procedure—supersedes all previous accounts. Equally significant is his description of what the Privy Council actually did in discharging its policy-making, administrative, and quasi-judicial functions. What emerges is a fresh picture of the contrasting regimes of the Protector Somerset and the Duke of Northumberland.

When Somerset overturned Henry VIII's will, which had designated a new Privy Council to govern England during Edward's minority, he restored the council to its traditional Henrician form. Moreover, the council of the Protectorate always had its fair share of Cromwellian administrators. The continuity, however, meant little because of Somerset's perfunctory use of the council. Policy did not originate in council as it should have during the King's minority; the council really did not advise as it would have under an adult monarch. Policy was actually made and acted upon by Somerset on his own. Though not discounting the importance of the failure of Somerset's policies, Hoak argues convincingly that administrative incompetence was the principal reason for his fall.

When Northumberland gained control, he packed the Privy Council, making it more Protestant and aristocratic and less Cromwellian in composition, but he also restored government by council. He used his powers as Lord President to dominate the council's proceedings, but he delegated the specialized work of government to councillors abler than himself. He gave Edward VI a

staged and controlled role in government. He also carried out a reorganization of procedure that for the first time fixed the administrative routine of the council. Northumberland merits credit for continuing and developing Thomas Cromwell's work of making the Privy Council the King's main instrument of government, but, as Hoak concludes, his treasonous and personally fatal attempt to alter the succession has obscured his achievement.

Hoak's book, which is incompletely outlined here, provides an important beginning for a needed revision of the prevailing interpretations of the reign of Edward VI.

MORTIMER LEVINE  
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MICHAEL VAN CLEAVE ALEXANDER. *Charles I's Lord Treasurer: Sir Richard Weston, Earl of Portland (1577-1635)*. Foreword by A. L. ROWSE. Chapel Hill: University of North Carolina Press. 1975. Pp. xvi, 261. \$18.95.

Most students of the events leading to the English Civil War have followed the Earl of Clarendon in viewing Sir Richard Weston as an ambitious courtier who, with success, became as overbearing in manner and grasping in spirit as he did corpulent in body. As Chancellor of the Exchequer in the later 1620s, he championed the king's needs at a time when the king's substance was being notoriously wasted; as treasurer during the first years of personal rule, he devised the means of keeping the government afloat without parliamentary sanction. To his own and later ages, these unpopular roles showed him to be an unprincipled time-server or the agent and apologist for tyranny. Now, in the first extended study of his career, Michael Van Cleave Alexander has made an attempt to see Weston as a statesman of talent and integrity who served his king diligently, working against great odds to restore prudence and responsibility to royal policy. As treasurer he increased ordinary revenues and kept the king's debts down by reducing pensions and preventing overseas military adventures. Though not a reformer or modernizer like his great predecessors Salisbury and Middlesex, Weston actually accomplished more than they, Alexander believes, simply by exploiting existing opportunities. Through his efforts the policy of personal rule would have succeeded—at his death the country was apparently placid, the government apparently solvent and its institutions apparently intact—had it not been for the absolutist instincts of his enemy, Archbishop Laud.

The materials from which Alexander has drawn can bear his sympathetic interpretation as easily as any other, and it is no doubt high time that the Earl of Portland's image be cleansed of some of the

tar that was liberally spread in the past. But Alexander is able to provide little that is new other than his tolerant understanding. There are no extensive personal papers from which he can reconstruct Portland's motives and sentiments with confidence, and too much must be accepted on the author's assertion. The lack of first-hand sources is a crippling problem throughout; too often Alexander is forced to summarize well-known events from familiar documents with no other purpose than to place Portland within them. But this book is useful despite its limitations; it provides a comprehensible survey of the functions of the major financial officers of Caroline England and a carefully plotted narrative of the career of the most important of them. A better treatment of either will probably have to wait for major discoveries of new sources.

ROBERT W. KENNY  
George Washington University

J. S. MORRILL. *The Revolt of the Provinces: Conservatives and Radicals in the English Civil War, 1630-1650*. (Historical Problems: Studies and Documents.) New York: Barnes and Noble, 1976. Pp. 234. \$15.00.

The wealth of attention devoted in recent years to the English revolution has included a large amount of research in local history, published and unpublished. The present work contains a good synthesis of studies in this field together with a selection of illustrative documents which should be useful to teachers and students. It also presents an interpretation of the revolution from a provincial perspective. Focused on the interaction between the central government and county communities, its theme is the persistent domination of local concerns, allegiances, and interests in the relation of the provinces to the center. J. S. Morrill calls this instinctive attachment and its concomitant political overtones the "pure Country" attitude. He sees it as the perennial element conditioning the response of provincial England both to the government of Charles I and to the revolutionary regime that succeeded. Because of it, the desire uppermost among many county notables and the mass of inhabitants during the Civil War was for the reconciliation of differences and peace. Away from Westminster and the capital, the prevailing note of the war period became one of opposition to the heavy burdens and unprecedented intervention from the center which hostilities brought in their train. Parliament, the author thinks, pretty much abandoned traditional methods of local government in fighting the war. The King, on the other hand, though trying to follow accustomed ways, could not control his forces, which ravaged the areas they controlled. But Parliament's financial

exactions and county committees introduced a new tyranny which set off a powerful provincial reaction. To drive home his argument, the author stresses the extent of neutralism during the war. He also gives a lengthy treatment to the Clubmen's movement in the West as the most significant manifestation of the neutralist trend, viewing it as a positive, clear-sighted force for moderation and the rule of law rather than as due to political indifference or sheer conservatism.

Critical readers are likely to feel some doubts about this account. They will consider that it carries its thesis too far and will question whether Parliament's rule in the 1640s was quite as untraditional, or neutralism as politically principled and inoffensive, as is suggested. They may also occasionally differ with the interpretation of the evidence for these conclusions. More generally, they may see in it, as I do, traces of the sentimentalism to which a good deal of local history has been prone through excessive stress on the independence, particularism, and autonomy of communities. To adopt the latter standpoint unqualifiedly is to overlook the fact that England was the most politically integrated and unified of all European kingdoms, that its provincialism was incomparably weaker than, as well as different from, for example, France's, that its aristocracy and governing elites were the most imbued with a national, kingdom-wide outlook, and that of all contemporary representative assemblies the English parliament alone, as the political focus of these elites, possessed both the ability and will to take over the rule of a great royal state.

In spite of these reservations, Morrill's thoughtful study is well worth reading and deserves praise as a serious effort to introduce a fresh dimension in the comprehension of the English revolution.

PEREZ ZAGORIN  
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MARGARET C. JACOB. *The Newtonians and the English Revolution, 1689-1720*. Ithaca: Cornell University Press, 1976. Pp. 288. \$14.50.

CHARLES WEBSTER. *The Great Instauration: Science, Medicine and Reform, 1626-1660*. London: Duckworth, 1975. Pp. xvi, 630. £13.50.

The history of science has long been torn by antagonistic ideals which have come to be summarized in recent years by the slogans "internal" and "external." Charles Webster's *Great Instauration* and Margaret Jacob's *Newtonians and the English Revolution* both aggressively embrace the external approach. Both interpret critical issues in the growth of English science in the seventeenth century in terms of religious and political movements. Both ignore the simultaneous developments in science

elsewhere, which to internalists always appear as important factors in any analysis of scientific change.

Webster's study reasserts the much-argued contention that the Puritans played a central role in the growth of modern science. Whether one agrees or disagrees with his argument, few will wish to deny the importance of this massive and exhaustive study, which is bound, I think, to be recognized at once as the leading exposition of the Puritan thesis. Where earlier works such as Robert K. Merton's, following the suggestion of Weber, have stressed the Puritan ethic, Webster adds a new dimension to the argument by showing the role of Puritan millennialism. Many English Puritans, including the extensive group clustered about Samuel Hartlib, the central figure in the book, thought the new age was at hand when England would build the Protestant New Jerusalem. The completed Reformation would usher in the great instauration, a revival of learning in which the fruits of experimental science would lead to a reconstruction of society. With a monumental display of scholarship, Webster follows the various ramifications of this vision. A chapter on the scientific community until the early 1660s is the best treatment of the background and organization of the Royal Society that has appeared. His discussion of proposed educational reforms places this literature, which has long been known, within a broader framework of comprehension. Finally, two long chapters deal with proposed medical reforms and with the allied topic of utilitarian projects in general. As the title of the book announces, the Puritans adopted Bacon's program of applying experimental science to the relief of man's estate; in the two concluding chapters, Webster details the multitudinous attempts to realize its promise in the period from 1640 to 1660.

The more important a book is, the greater the obligation to discuss it critically. Modern science has been at least two things. On the one hand, it has been a sociological phenomenon, a movement involving an ever-increasing number of men. For a century now, statistics have been compiled which show the concentrated role of Calvinists in the scientific revolution. Analogous statistics, which further extend the implication of earlier ones, provide the bedrock on which Webster's position rests. A century of criticism has not conjured earlier statistics away; I suspect that Webster's will also survive. Science is also a second thing, however, a body of theories. To internalists, a discussion of the growth of modern science that has nothing to say about the specific restructuring of natural philosophy in the seventeenth century is radically incomplete. Perhaps the ultimate question between Webster and the internalists is the

application of the word "science." With some disdain, he quotes Rupert Hall's opinion that Puritans wrote about science in general terms but did little to advance it. "Distinctions of this kind," he asserts, "have no real historical foundation." Unfortunately, his pronouncement comes after nearly five hundred long pages crammed with talk about science in general but very little in particular that many would call science. Can one seriously consider John Beale's garrulous descriptions of how they grew fruit trees in Somerset as science? Does one find in Beale—or in proposals to coin with machines (the principles of which had been known for centuries), in mercantilist justifications of the Navigation Act, or in the Down Survey of Ireland—that radical revision of the categories of natural science without which the modern world would be an utterly different place? The issue is not the worthiness of these activities, but whether clarity of thought is served in calling them science.

Similar considerations apply to technology. Webster's repeated reference to the modern separation of science from technology, in contrast to their union in the seventeenth century, utterly baffles me. I see things exactly reversed. In the last hundred and fifty years, science—that is, the specific intellectual product of the scientific revolution—has penetrated and transformed technology to the point that it is now virtually impossible to distinguish the two. The seventeenth century, on the other hand, was filled with talk about useful knowledge which had, as far as I can find, little bearing upon actual technology. When one considers how central it is to his argument, it is revealing how few useful inventions Webster can trace to his Puritan scientists. A great deal is said about the diffusion of established techniques (which arose initially from empirical practice which can hardly be called scientific). One new dye is the only useful product of scientific investigation that I found in the entire book; even in that case I would be interested to learn whether in fact it came into general use. I am not ready on so slender a basis to find the source of modern scientific technology in the Baconian talk of Puritan reformers.

Despite these reservations, I regard *The Great Instauration* as an important book. It is a major contribution to the historiography of the Puritan revolution. In his desire to make it also the explanation of the scientific revolution, however, Webster stretches his case beyond its limit. Surely we need his attention to the scientific movement, but his disdain for the internal history of science is no less one-sided than the lack of attention to the movement by others.

In shorter scope, Jacob's book aspires as grandly. A study of the Latitudinarians, it discusses their use of Newtonian natural philosophy

in social philosophy, specifically as they strove to find a formula for reconciliation after the Glorious Revolution. If Jacob were willing to leave it at this, one could welcome her work as a penetrating investigation of the Latitudinarians' practical theology. As her label "the Newtonians" announces, she is not ready to leave it there, however. She claims to discover the source of Newtonian natural philosophy in the ruminations of the Latitudinarians. I am puzzled by her principles of historical confirmation. Without even examining the scientific roots of Newtonian natural philosophy, she is prepared to pronounce confidently that its source is to be found in the group she has studied. In light of the significance of Newtonian natural philosophy, her argument implies that the Latitudinarians were crucial figures in the intellectual history of modern Europe. Whenever I try to take them that seriously, I recall the seventeenth century response to the question of what the Latitudinarians held: "The richest benefices in England."

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JUDITH HOOK. *The Baroque Age in England*. London: Thames and Hudson; distributed by Transatlantic Arts, Levittown, N.Y. 1976. Pp. 207. \$19.50.

Judith Hook has attempted an introduction to the cultural history of England between 1630 and 1730. The theoretical impetus for the book is sound, that a historian should take into account society's "characteristic forms of self expression both conscious and unconscious" (p.7); but the title and the book's claims are misleading. Most of the book is on exterior and interior architectural material, with some intellectual, political, and social background. The plates, mostly of exteriors and interiors of buildings, are well-chosen and functional. The chapter on "The Artist in Society" is interesting and useful. So are chapters such as "Money and Materials" and "The Makers [artisans] of the English Baroque." Hook is especially good on the historical details of what expenses and work habits went into building and decorating. Even the bibliography and appendixes primarily concentrate on architectural studies. Appendix II, for example, is "an Alphabetical List of Some Major Baroque Works of Architecture" (in England). There is no comparable list for any of the other arts.

There are some nagging problems. Although she designates a time as "baroque," Hook never defines (only describes, pp. 11 ff.) the kind of art she calls "baroque." B. Sprague Allen's *Tides of English Taste* is dated 1969 although it first appeared in 1937. Danniells [sic] is misspelled. Bredvold's *In-*

tellectual Milieu of John Dryden (1934) has been superseded by Philip Harth's *Contexts of Dryden's Thought* (1968). One would expect more footnotes pointing out specialized primary and secondary material. Hook, omitting vast areas of scholarship and seventeenth-century writing, includes twenty-six authors in the list of primary sources, and only sixty-three secondary works. She also says, "There were no theoreticians of English baroque art" (p. 9). Her statement is true only insofar as there were no elaborately worked out esthetic philosophies. But there was much theory expressed in prefaces, essays, and translations. To indicate inadequately the amount of theoretical writing, Thomas Pope Blount's *De Re Poetica* (1694) cites the names of 124 English writers at least twice each. Besides, Hook omits the flood of translations of continental theorists on all the arts, as listed in Wing's *Short Title Catalogue* (1945) and *Gallery of Ghosts* (1967). Because of the English culture and language, these translations were anglicized. One example is John Evelyn's translation of Roland Fréart's *Idea of the Perfection of Painting* (1668), where Evelyn disagrees with Fréart's evaluation of Michelangelo. Another is Boileau's *Traité du Sublime* (1674) where because of cultural differences the English concept of the sublime came to mean something quite different.

A more serious fault is Hook's tendency toward over-simplification. For one example, in the chapter, "World Views" Hook fails to explain the differing conceptions of the human soul (pp. 109-10) on which the different conceptions of reason depend. Her brief précis of the positions of the Cambridge Neoplatonists and the Cartesians is sound, but she misrepresents Hobbes' materialism, making his arguments on fancy fit her conception of baroque art, saying "No argument could have come closer to the central assumptions of baroque sensibility" (p. 118). Such an argument is misleading. The fancy forms the rhetorical devices which persuade by moving the emotions and fancies of viewers and auditors, but Hobbes designates fancy as dependent on memory, as "decaying sense." He designates fancy inferior to judgment, or knowing, and since it is the important faculty in artistic creation, art becomes inferior to science and history, which deal with facts. In what we think of as baroque art (say by Bernini, Pozzo, Vivaldi, Crashaw, or Milton), fancy (or imagination), judgment, and rhetorical manipulation are combined. The imaginative visions produced by such artists, writers, and musicians defy Hobbes' circumscribed idea.

Hook has the liberty to choose her own subject and approach, but we as readers should expect a well-defined subject which leads to logical conclusions. In this case, the subject is not clearly defined, and although there is interesting and in-



formative material, the claims and conclusions of the book are relatively empty.

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PAUL M. KENNEDY. *The Rise and Fall of British Naval Mastery*. New York: Charles Scribner's Sons. 1976. Pp. xviii, 405, \$15.00.

Paul Kennedy offers this book as the first detailed reconsideration of the history of British sea power since Mahan. Where Mahan wrote because he believed that sea power had been "vastly underrated," Kennedy argues that in fact the "classical age of sea power" was relatively short, lying between the revolution occasioned in world politics by the remarkable evolution of the European sailing ship from the fifteenth century and the industrialization of the European, Russian, and American continents from the later nineteenth century. Exceptional circumstances such as the diversion of so much of the potential strength of Britain's European rivals to continental conflict, Britain's emergence as the world's first industrial nation, and the extraordinary weakness of the non-European world for at least two centuries are needed to explain Britain's rise. Its fall is less surprising—"a classic example of what Liddell Hart once called 'strategic over-extension'."

Kennedy analyzes these developments with lucidity and skill. One example of the general soundness of his judgment must suffice. On British policy in the 1930s he observes: "It would, however, have taken a prophet of rare sagacity and insight to have foreseen that the international situation would change so swiftly, and that so many simultaneous threats to the British Empire would occur precisely at a time when its defences were weakest and when the public's attention was riveted upon the domestic scene." The author does assume that Britain could have disengaged more easily than it did from world affairs after 1945. More thought was given by the government in 1946-47 to the burdens of a continuing imperial role than he recognizes, and one of the effects of the close alliance with the United States from 1947 was to encourage Britain to bear until the later 1960s commitments which were really beyond its strength.

Kennedy provides ample references and a good index, though it is a pity he chose to reduce the value of the bibliography by confining it to the works actually cited in the references. Ample attention, however, is given to the contributions of economic historians, which provide a valuable corrective to the assumption of many naval historians that Britain should and could bear almost any level of naval expenditure. If only one-third of the book is devoted to the period down to 1793, while the period from 1859 receives no less than half, this

is perhaps fair enough given the recent studies of the earlier centuries by Geoffrey Marcus. Students of both naval and international history will find this a most stimulating and thoughtful work.

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JOHN BREWER. *Party Ideology and Popular Politics at the Accession of George III*. New York: Cambridge University Press. 1976. Pp. ix, 382. \$27.50.

John Brewer has produced an immensely important reassessment of the relations between politics and ideology in the 1760s. He contends that by the 1760s a national communications infrastructure had emerged sufficiently to produce a deeply rooted and well-informed political culture, in the countryside as well as in the metropolis. This new political culture, which he terms an "alternative structure of politics," was "predominantly bourgeois and characteristically commercial," though he often seems to suggest it penetrated the country gentry and freeholding classes as well. During the 1760s this new political culture was given "focus" by the issues of Wilkes and America, thus irreversibly politicizing the consciousness of a "second political nation" which then began an ultimately decisive challenge to the exclusionist and aristocratic political system of Augustan England. Indeed, the configuration of high politics itself underwent radical alterations and became unstable as the bases of the old order collapsed ca. 1760. Brewer offers a comprehensive and strikingly coherent, though contentious, interpretation of the entire spectrum of political thought in the period and, by treating ideology as legitimation, reinstates it as a central basis for understanding political action.

Brewer is at his best when dealing with national politics and the upper levels of political society. But when he moves into the countryside he displays serious deficiencies for an author whose central purpose is to relate ideology to situational behavior. His discussion of communications infrastructure omits any mention of a developing internal market economy. He seems ignorant of central place theory. His countryside is a bivariate plane loosely described as rural and "urban"—that damnably imprecise word. No mention is made of farming regions, no distinctions are drawn between an aristocracy of renters and leaseholding farmers, or between types of artisans and shopkeepers, or of the overlapping relations between these and other single "interest" groups. We hear only of "gentry" or "merchants" and the like. The undifferentiated countryside of Brewer never existed. Brewer's perspective is good so long as it is confined to the traditional subject matter of political history. But when he leaves the metropolis and

country house and ventures into chalk and cheese. he brings with him the naïveté of the traditional historian of ideology.

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TRAVIS L. CROSBY. *Sir Robert Peel's Administration, 1841-1846*. Hamden, Conn.: Archon Books. 1976. Pp. 190. \$12.50.

Travis Crosby has written a handy and useful addition to the Elections and Administrations series launched by David and Charles several years ago. The original aim of the series (now unlikely to be realized) was a fresh appraisal of British governments from 1815. Crosby has distinguished predecessors in J. E. Cookson, *Lord Liverpool's Administration, 1815-1822* (1975) and A. K. Russell, *Liberal Landslide: The General Election of 1906* (1973). This volume is clearly not on the same scale as those that have gone before, nor does it break the new ground they did; but this is probably largely to be explained by financial exigencies, rather than by any deficiency in the author. Crosby's book must be judged on its own merits, which are many.

It is a brief, concise, and balanced survey of Peel's second government. Crosby does not, and never claims to, advance any new and startling interpretations. Rather, on the basis of extensive reading in secondary works as well as in the main manuscript collections, he attempts to make an up-to-date appraisal of the achievements of the government and to place his interpretation firmly in the context of current historiographical trends and controversies. Thus he feels that the great social and economic reforms that dominated the government's domestic policy are better accounted for by the empiricist explanation than by the theorists' stress on Benthamism. He finds the ministry's foreign and imperial policy—though he argues that its sound and pacific intentions have not been sufficiently appreciated—fitting better the notions of Ronald Robinson and John Gallagher than those of Oliver Macdonagh. Crosby's cool and dispassionate survey of these aspects of the government's policies, as well as of the Irish situation, is a most useful summing up of the present state of scholarship.

He makes his most original contribution in his first and last chapters, where he places Peel in the broader context, not only of parliamentary but also of constituency politics. Crosby knows a great deal about the farmers' protest movements of the first half of the nineteenth century. He therefore appreciates and conveys better than most of his predecessors the sense of outrage and betrayal felt by a large and significant proportion of the British electorate which had gone to the polls in 1841 to

vote for Peel and protection. One wishes there had been space for more on this.

Peel's great contempt for his own party is well known and clearly demonstrated by Crosby. His proposition that Peel had a similar contempt for broader public opinion is more questionable—certainly of the author of the Tamworth Manifesto and probably of the minister who repealed the Corn Laws. Peel himself, as Crosby points out, gave the primary credit for the latter accomplishment to Cobden, who had been mainly responsible for rousing public opinion, and said more than once that his own great aim had been to still the resulting conflict. But if Crosby follows the current fashion of seeing Peel as perhaps a trifle more Olympian than he was, he is in excellent company. One can sympathize with those who find it difficult to restrain their admiration.

RICHARD W. DAVIS  
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DEREK FRASER, editor. *The New Poor Law in the Nineteenth Century*. (Problems in Focus Series.) New York: St. Martin's Press. 1976. Pp. 218. \$16.95.

For a long time students of English history considered two facts about the New Poor Law of 1834 to be incontrovertible: it centralized the administration of the poor laws, and it imposed the infamous workhouse test on the poor. Sydney and Beatrice Webb, in their *English Poor Law History*, published in 1929, painted a picture of three grim Commissioners in London issuing orders only a bit less peremptory than a tsar's ukase, while in the new unions equally grim guardians of the poor told paupers, either enter the workhouse or no relief!

Since 1960 scholars such as Rhodes Boyson, Nicholas Edsall, and Ruth Hodgkins have greatly modified that story. In the *New Poor Law in the Nineteenth Century* Derek Fraser and seven other contributors have used those findings and their own research to present a fuller and more balanced picture. The Webbs, of course, were not entirely wrong about the Law's centralizing features and the harshness of the workhouse test, but they did underestimate the strength of the local unions and their resolute defiance of the workhouse test.

David Ashworth in his chapter on "The Urban Poor" and Ann Digby in her "The Rural Poor" make no such oversights. Ashworth finds, for example, that in most years during Victoria's reign 85 to 94 percent of all urban paupers on relief received that relief outside the workhouse. Most workhouses, in fact, became largely asylums for the care of children, the aged, the sick, and the infirm. The proud, jealous, testy guardians of urban unions were anything but slaves to London. Some guardians even gave relief to those already

earning wages, rank heresy according to New Poor Law orthodoxy. Rural guardians also resisted the workhouse test. Ann Digby writes of the emptiness of workhouses in the 1860s and of the very high ratio of outdoor to indoor relief. The central board, she concludes, "was impotent to enforce its relief policies."

The Poor Law Commissioners and, after 1847, the Poor Law Board not only could not enforce the workhouse test but could not persuade local guardians to provide good schools and an adequate health service. Francis Duke, in a chapter on "Pauper Education," finds that most unions either totally neglected the education of the children or paid a badly trained teacher a paltry sum to hold classes in demoralizing conditions. Little was impressed on the pupil's mind but the stigma of pauperism. The greatest barrier to good schooling was its cost. The guardians and those who elected them feared nothing so much as higher rates.

Michael Flinn shows that this fear also lay behind the refusal to provide infirmaries, employ able doctors, hire nurses, and send the mentally ill to asylums. Local parsimony even extended to towels: the Paddington workhouse provided only one for every twenty-seven patients.

Michael Rose also emphasizes the power of local forces in his chapter on the settlement laws. Under these laws a large urban union could tell those whose actual settlement was elsewhere that if they applied for relief they would be removed to their former parish. Such threats, since they deterred many from seeking relief, saved urban unions money. Rural parishes also liked the settlement laws because it meant they were responsible only for those few paupers settled in their parishes. These two local forces, one urban and one rural, raised insurmountable barriers to the reform of these cruel and restrictive laws.

*The New Poor Law in the Nineteenth Century* is not a definitive study. It is often thin, sometimes repetitive, and on occasion irrelevant. But because it focuses so well on those powerful local forces that actually defined the working of the poor laws it is a most valuable study.

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LEE E. GRUGEL. *George Jacob Holyoake: A Study in the Evolution of a Victorian Radical*. Philadelphia: Porcupine Press. 1976. Pp. x, 189. \$12.50.

At times Lee E. Grugel's claims for his subject are so modest that he undermines the justification for a new biography. Holyoake was "not a commanding figure on the lecture platform"; his first significant book was "dull and stale"; he rarely aroused "sustained enthusiasm"; even his contribution to

his favorite cause, the cooperative movement, "was less than that made by other men." Holyoake was not much of a businessman or an organizer either, but he was a skillful journalist, and the "secularism" he defined and professed "correctly described the intellectual consciousness of the emerging labor aristocracy" (p. 99).

George Jacob Holyoake (1817-1906) survived into Edwardian England as a last living link to the Chartism and the Owenism he had both espoused and criticized in the 1830s and 1840s. By then his six-month imprisonment for blasphemy in 1842 must have seemed an exotic aberration, for Holyoake proved to be a moderating force in both the English free thought and political reform movements, and in his later years he corresponded frequently and amiably with Gladstone. Edward Royle's *Victorian Infidels* (1964) provides a much fuller account of Holyoake's secularism, and David Tribe's *President Charles Bradlaugh M.P.* (1971) throws more detailed light on the Bradlaugh-Holyoake rivalry, but Grugel has provided a readable brief survey that is based on solid research in Holyoake's unpublished and printed writings and that displays familiarity with the secondary literature.

The book is weakened by a lack of clarity in regard to the social structure of Holyoake's England. Grugel views his subject alternately as a representative of the working classes, "the men of factory and mill," and as an apostle of the "labor aristocracy." Yet, in other places, Grugel concedes that, by any socioeconomic standard, Holyoake was a "petty bourgeois." Perhaps the key problem is that the Victorian social structure does not truly lend itself to Disraeli's famous description of "two nations . . . the rich and the poor." If one does not assume a pervasive working-class consciousness, one is less likely to be surprised that "apathy, ingratitude and hostility more often than not were the prizes which the working class awarded to its leaders and educators" (p. 48).

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JO MANTON. *Mary Carpenter and the Children of the Streets*. London: Heinemann Educational Books. 1976. Pp. xii, 268. £9.50.

Jo Manton's study of Mary Carpenter (1807-77), the redoubtable champion of "ragged children," is a long overdue consideration of one of the most important social reformers of the high Victorian period. Carpenter, like so many Victorian women, was not free to pursue her own vocation—educational reform for the poorest children of her time—until her father died in her middle age and freed

her both economically and psychically. Once she began her reform work, however, her energy and self-sacrifice were almost limitless. After the success of her free school in Bristol she went on to organize agitation for state aid, and increased philanthropy, on behalf of the poor children.

Brought up in the tradition of Unitarian radicalism, her most lasting contribution to educational and reform theory was her definition of delinquent and poor children as innocent and neglected, rather than precocious adults in need of punishment for their transgressions. While far ahead of her time in her theories about children, she was never so Victorian as in her methods of seeking social change. She eschewed all connections with the new advocates of feminism and instead pursued a course of individual action based on pressure from within. Through sheer force of will—and a good deal of hectoring—she repeatedly pressed M.P.'s, government bureaucrats, and various philanthropic organizers to carry out her programs. As reforms were gradually instituted (the 1889 Prevention of Cruelty to Children Act came out of measures she had inaugurated half a century earlier) Carpenter's interests branched out to include the treatment of adult prisoners, the education of Indian women (she made four trips to India in the last decade of her life), and industrial day schools.

Manton's book is an absorbing account of Carpenter's life. She has an eye for biographical detail and has made good use of the extensive Carpenter papers that have survived. Moreover, she has managed a very good balance between a psychological analysis of Carpenter's motives and a consideration of her actual deeds. The social historian, nevertheless, would perhaps have preferred a different approach to the issues Manton raises. While the life of Mary Carpenter is clearly delineated, the wider issues raised by her theories and practices are often lost in personal details. There could be much more discussion of Carpenter's relationships with other reformers who followed her or pioneered in related fields. The questions of why reformers from the mid-fifties were concerned with ragged children and what theories of the family they espoused are skirted without sufficient discussion. Although Manton repeatedly asserts that Carpenter was anxious to preserve the family and to keep children in their homes, she does not analyze why Carpenter spent so much of her time and energy on boarding schools, not all of which were intended as reformatories. Indeed, like Carpenter, Manton does not always distinguish clearly between the innocent ragged child and the delinquent. Since Carpenter made so much effort to argue for their equal innocence, this confusion is not surprising, but it points to a weakness of the

book. The biographical form has not permitted Manton sufficient scope for placing Carpenter in her period.

Despite these strictures, *Mary Carpenter and the Children of the Streets* is an eminently readable account of the power of one woman to obtain reforms for those less fortunate than herself. Since so many of the issues she confronted remain with us today, her life is a sobering reminder of the difficulties of reform even in a confident and expansive society.

MARTHA VICINUS  
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RICHARD GURNHAM. *A History of the Trade Union Movement in the Hosiery and Knitwear Industry, 1776–1976*. Leicester, England: Leicester Printers, for the National Union of Hosiery and Knitwear Workers. 1976. Pp. xiii, 197. Cloth £6.00, paper £2.50.

One wonders whether it is significant that among British trade-union histories it is the small and relatively conservative organizations which have chosen to commission academics to research their origins and development, employing the scholarly standards and critical apparatus which in themselves ensure their lasting value to historians. Richard Gurnham's study is one of these.

The treatment of the first one hundred years of the hosiery trade unions' discontinuous existence relies heavily upon existing secondary work, and it is disappointing that when he encounters disagreement Gurnham sometimes declines to adjudicate. The major themes are the attempt by the Midland Counties unions, centered in Nottingham and Leicester, to organize hosiery workers in the low-wage villages and, from the beginning of the twentieth century, by which time the villages were at last coming under the control of the urban unions, the organization of the more remote and even more recent production centers in Scotland, Lancashire, Yorkshire, and the Home Counties. This was a campaign which seemed to have achieved success in 1945 with the formation of the National Union of Hosiery and Knitwear workers. But since that time recruitment in these areas has proven difficult and in the 1970s is still an uphill battle. Organization, the drive toward amalgamation, and national negotiation, are the major topics in this history, and, although there are occasional references to wage agreements and relative standards, there is no sustained analysis of the unions' achievements except in terms of membership. Nor, for the twentieth century, is there a convincing attempt to place the history of the union in a wider economic context.

Gurnham's study, originally a doctoral thesis,

will no doubt be heavy going for the ordinary Nottingham knitter, but the thoroughness of research and lucidity of narrative and analysis combine to produce a rewarding study—within the limits of his chosen frame of reference.

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HENRY WEISSER. *British Working-Class Movements and Europe, 1815–48*. Manchester: Manchester University Press. 1976. Pp. viii, 226. \$17.50.

This work attempts to deal with early British working-class movements, the growth of class consciousness, national and international awareness, and the ways in which certain groups of British people looked on Europe and were affected by it. These are, to say the least, less than concrete subjects, which do not lend themselves to precise analysis or mathematically accurate evaluation. Given this, and the nature of the sources, perhaps what the author is trying to do cannot be accomplished in any entirely satisfactory way.

Inevitably, much of the material is drawn from the working-class newspapers and pamphlets of the period. When their information cannot be checked against other, harder data, such sources tend to generate a sense of unease in the reader, but Henry Weisser handles them with commendable caution and modesty. His approach is sound in its lack of grandiose claims, but frustrating in its dearth of analysis and conclusion. Especially interesting are the relationships between class consciousness, class resentments, and attitudes about European issues. Was xenophobia generated more by a dislike of foreign ruling classes than by a sense of national identity? Were working-class opinions about certain foreign policy issues shaped more by a dislike of Palmerston than by the issues themselves? Such questions are touched on, and a good deal of information is supplied, but the author does not seem quite able to bring himself to suggest answers to them. For example, on what might have been one of the central analyses of the book, the interaction of British and European working-class thought and movements, the only conclusion offered is that “who led whom is, of course, a moot point.”

There is a short historiographical essay on Chartist internationalism which the author concludes with the hope that his work may help clarify the judgments of other historians. If this was indeed his purpose, it might have been a little better served had he offered more positive judgments of his own.

JANET ROEBUCK  
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ROBERT Q. GRAY. *The Labour Aristocracy in Victorian Edinburgh*. Oxford: Clarendon Press. 1976. Pp. ix, 220. \$17.50.

This book is a good example of the importance of local history, not merely as a source of new detail, confirming or modifying established conclusions, but as a means of achieving a fuller reconstruction of social phenomena in their complexity and interconnectedness. Robert Gray's monograph is noteworthy for the precision and the richness of texture of its analysis of the skilled workers of Edinburgh and of the socioeconomic circumstances that shaped them.

The main body of the book begins with an account of the economic characteristics of the labor aristocracy in Edinburgh in the second half of the nineteenth century: production processes, wages, standard of living, and variations in work experience. An especially valuable chapter on “Community and Social Status” includes a precise specification of social differentiation by reference to patterns of residence, marriage, and participation in voluntary organizations, along with a discussion of the artisans' relations with nonmanual groups. Two chapters deal suggestively with values and attitudes, among them respectability. Then the author turns to trade unionism and politics. Throughout the book Gray is attentive to causal links and interconnections and makes very good use of sociology.

Gray notes that his argument “is located within the Marxist tradition of socio-historical analysis.” The book reflects the theoretical and analytical vitality of that tradition. It also is firmly committed to a Marxist interpretation, composed of two interlocking components, presented chiefly in the first and last chapters. Following the formulation first advanced by Lenin and subsequently refined by Hobsbawm, the author treats the labor aristocracy as a distinct “social formation” created by the economic structure of a maturing capitalism; as such it is sharply distinguished conceptually from the skilled artisans of the first half of the nineteenth century. Secondly, Gray argues that this labor aristocracy provides the key to the explanation of the reformist and nonrevolutionary behavior of the industrial proletariat. The argument is unconvincing, however, because there is really no need to search for an elaborate explanation of why the proletariat did not become revolutionary. The notion that the proletariat was endowed with an inherently revolutionary character was a product of Marx's wishful thinking in the utopian atmosphere of the 1840s. Moreover, since his book begins in 1850, Gray is not able to demonstrate that there was a fundamental difference, attributable to economic change, between the



skilled artisans of the first half of the century and the "labour aristocracy."

Even the reader who is unable to accept Gray's thesis, however, will find his subtle and perceptive analysis most illuminating.

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Columbia University

RONALD HYAM. *Britain's Imperial Century, 1815-1914: A Study of Empire and Expansion*. New York: Barnes and Noble. 1976. Pp. 462. \$20.00.

BERNARD PORTER. *The Lion's Share: A Short History of British Imperialism, 1850-1970*. New York: Longman. 1976. Pp. xiii, 408. Cloth \$18.50, paper \$9.00.

It is interesting to observe how the revisionist thesis, argued particularly by John Gallagher and Ronald Robinson, has now become virtually the accepted canon of recent historians of modern British expansion. These two books are strikingly different in character. Ronald Hyam's account is a detailed and systematic narrative, commenting with a good sense only occasionally marred by a tedious flippancy on the economic, humanitarian, religious, strategic, and cultural factors that created Britain's last imperial century. Bernard Porter's study, starting later, but continuing on until the mid-twentieth-century end of empire, is much more impressionistic, freer in some of its generalizations perhaps, yet more cautious in its attributions of motive and of purpose. Both, however, share the Robinson and Gallagher rejection of the notion that empire was methodically pursued from Whitehall. They stress instead the interplay of politicians, overseas governors, military officers, and assorted other groups in the complex processes that led to the growth—and in the case of Porter's account, the decline—of empire. But neither book is a slavish rehearsal of older interpretations. Each emphasizes a few themes which give a certain distinction to the narrative and make it a contribution to our evaluation of the imperial experience.

Hyam divides his work into two parts. Starting with a chronological history of the nineteenth-century empire, he then turns to the various areas in which its development occurred—America, India, Egypt, Africa, the Antipodes, and East Asia. A final section draws conclusions about the character of British colonialism and suggests reasons for its collapse.

Chief among his conclusions is the contention that the government was *never* (his italics) manipulated by economic pressure groups, which indeed could not agree among themselves. It generally ignored trading interests even when their activities

were directly concerned in a change of policy, as in Tunis in 1878 or Persia in 1907. Instead, he maintains, territory tended to be acquired when two levels of interest interlinked, that is, when individuals overseas created circumstances which made an acquisition possible or probable and when Whitehall decided to act because of pressures generated by an international rivalry attributable not so much to colonial competition as to domestic urges toward expansion within Europe. And, he points out, even the coincidence of interest by no means automatically produced territorial control.

Such conclusions are relatively conventional and—although they stress insufficiently the general economic atmosphere of the great days of expansion—reasonably persuasive. When Hyam also concludes, with little preliminary evidence beyond the fairly obvious (Gordon of Khartoum, Roger Casement, Cecil Rhodes), that the "ferocious effort put into acquiring and running an empire was directly proportional to the frustrated, repressed, and unimaginative love-lives of its heroes" one may be piqued but hardly convinced. There would seem to be nothing uniquely British about the sublimation of sexual and religious energies in such secular activities as service to the empire. To be sure, the expatriate Briton, like his European counterparts, behaved differently abroad—theatrically, as Hyam puts it—than at home. But it places matters out of perspective to spend more time on this one issue in a conclusion of a few pages than on virtually any other aspect of imperial expansion.

Hyam devotes a page and a half to the end of the empire. Porter makes it almost the dominant theme of his book. He argues, as others have suggested before him, that "imperialism" was for Britain a symptom and an effect not of strength but of decline in the world. The fear that Britain was falling behind led to the hope that the key to survival in an age of continental superpowers might lie in the empire. Until 1939 that vision of Britain's supremacy being salvaged by a judicious handling of imperial resources was, if an unlikely aspiration, not completely unrealistic. Even more than Hyam, Porter sees that the end of empire was prefigured well before the end of the nineteenth century, perhaps from about 1870. Along different lines, he also suggests that the empire played a major role in obscuring, as it aggravated, the long-term malaise in the British economy that set in around that date. And, in a way agreeing with Hyam, he sees that the empire was "controlled" very much less by Britain than it controlled her. In order to hold on to it she again and again compromised her freedom of action, in the end losing the empire anyway.

Some of these themes reflect emphases that Por-

ter does not share with Hyam. But differences in some conclusions, and in the styles of the two authors, only highlight the similarities in the two studies. Like Hyam, Porter sees "conquest forced upon us" or "reluctant imperialists." He agrees as well that just as the capitalist's world was wider than that of the statesman, so, in other directions, was the statesman's world much larger than that of the capitalist. To be sure, a large part of Porter's account deals with the years of devolution. The empire, he contends, was a cushion to soften Britain's fall in the world. From 1870 to 1970, her history was one of almost unbroken economic, strategic, and political decline from the halcyon days of the mid-nineteenth century. The loss of empire merely confirmed a deterioration that set in as Britain's early industrial supremacy was challenged by others.

Here, then, are two intelligent general works, solidly based on the scholarship of the past several decades. While they differ in some areas, both insist that the history of modern British imperialism is more complex, more haphazard, and thus perhaps more intriguing than ideologues of whatever stripe would have us believe. And their insistence is persuasive.

HENRY R. WINKLER  
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DON M. CREGIER. *Bounder from Wales: Lloyd George's Career before the First World War*. Columbia: University of Missouri Press. 1976. Pp. vi, 292. \$12.50.

The Lloyd George industry reached the takeoff point some years ago. Ever since the opening of his private papers to historians in 1967, essays and monographs on the great Welsh radical have poured forth in profusion. Several biographies are already under way, and now Don M. Cregier has produced a full study of Lloyd George's career until the outbreak of war in 1914, apparently the first volume of a trilogy.

The author confesses in his preface that he has no radical revisions to offer, and there is an air of *déjà vu* about much of the book. We have read so often of the early Odyssey of the "cottage-bred boy," the school days by the river Dwyfor, the influence of Uncle Lloyd, the early adventures in the county courts, the Llanfrothen burial case, the final electoral triumph at Caernarvon Boroughs, and so forth, that it is not surprising that Cregier has very little to add in terms either of source material or of interpretation. Indeed, the account of Lloyd George's activities down to the 1906 election seems to lean heavily on the published works of previous authors, while the range of manuscript materials used is relatively limited. The book becomes more interesting when Lloyd George enters the Liberal government, although it is disappoint-

ing that the author has so little to add on the period at the Board of Trade (so vital in Lloyd George's relations with organized labor, for instance) and that he has failed to use the Treasury papers in discussing the People's Budget.

Even so, the book has considerable merits. It is pleasantly written and (apart from one or two minor lapses on Welsh matters) commendably accurate. It is sane in its judgments on such themes as Lloyd George's attitude toward empire, free trade, and social reform. It has some interesting observations on the period 1910-14, which occupies almost half the book and seems to engage the author's attention more closely. The discussion of the "coalition" moves of 1910 (Lloyd George's version of Theodore Roosevelt's New Nationalism), of the land campaign of 1913-14, of the Ulster crisis, of defense and foreign-policy issues is always lucid and well worked out.

This is, in general, a modest book. It does not offer any general view of the politics of the time, whether, for instance, Liberalism before 1914 was thriving, or in decay as Dangerfield once believed. It does present a fair-minded, clearly constructed account of a career crucial to the understanding of twentieth-century British domestic and foreign politics. That it does so without muckraking unduly into Lloyd George's private life, which recent filial and other works have so inflated and distorted, makes Cregier's book all the more welcome. Historians will look forward warmly to his subsequent volumes.

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Oxford*

SEAN GLYNN and JOHN OXBORROW. *Interwar Britain: A Social and Economic History*. New York: Barnes and Noble. 1976. Pp. 276. \$15.75.

The *Economist* called this excellent volume "perhaps the best introduction to the economic history of the origin of our present discontents." The authors include current scholarship, discussing articles and using sources as recent as 1974. Their coverage is admirable—economic growth and living standards, overseas trade, industrial development, economic policy, unemployment, labor, population, housing, and social policy. The text is packed (not cluttered) with circumstantial material; there are thirty-three tables; statistics abound. Yet the result is not a catalog since analysis and argument outweigh mere fact. Most chapters begin with a compact homily on basic theory. Where problems of method bedevil researchers, Sean Glynn and John Oxborrow elucidate, showing why confident summations must wait.

No year-by-year narrative is supplied; basic historical knowledge is presumed. Nor does the work

produce one overriding judgment, moral or otherwise. The reason is fundamental. The authors contend that unemployment, and also rather rapid economic growth, were both realities. They see truth in both what the people remember—"depression and distress"—and what academia now emphasizes—"economic progress, with rising material standards" (p. 13).

Despite this deliberate balancing, the authors lock horns with the experts. Two examples may serve. First, did Britain fail to cope with crisis in 1931 because it rejected Keynes' sound advice? They temper one assertion, "we now know that Keynes was right," with a warning not to assume that "absolute truth and light confronted folly and ignorance." Since Keynes' ideas were not fully formed in 1931, orthodox Treasury views seemingly provided "a more coherent, logical and consistent basis for policy" than his schemes. Second, did Britain's interwar decline result from continued reliance on staple export industries, when it should have invested in new modern industries? Rejecting this easy dualism ("staples" v. new growth industries), Glynn and Oxborrow favor a perspective founded on multiple judgments: old staples showed considerable efficiency and innovation; some new industries showed below-average productivity gains; absolute decline was rare; growth performance over all was appreciable.

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RICHARD CROSSMAN. *The Diaries of a Cabinet Minister*. Volume 1, 1964-1966. New York: Holt, Rhinehart and Winston. 1975. Pp. 688. \$16.95.

Speaking to Richard Crossman some years before the events described in this book, I praised the work of a dedicated Labour backbencher. "Ah," Crossman replied, "but think how much more good one could do in a position of power!" This book is a first installment of his experience of power.

Let it be said at the outset that it is a riveting book, fully justifying the controversy which surrounded attempts to prevent or delay its British publication. Crossman had a reputation for verbal extravagance, but his diary has the ring of truth. If it is even half true, or if even half of it is true, then the book is undoubtedly the most revealing as well as the most interesting account of British government from the inside ever written.

One principal theme is Crossman's relations with his senior civil servants and above all his permanent secretary at the Ministry of Housing, the formidable Dame Evelyn Sharp. Crossman was arguably the most intelligent, energetic, and imaginative member of a government not overabundantly endowed with these qualities. Yet he

came off worst in most of his battles with his civil servants, whose primary loyalty was to their masters at the Treasury rather than to him. As a result of this and other institutional and human factors, the civil service rather than government ministers dominated much of the decision-making process. Crossman's principal opponents were neither his own backbenchers, who played a small, enthusiastic, but ill-informed part in proceedings, nor (despite a virtually nonexistent Labour majority) the Conservative opposition, but rather his own civil servants. One is brought to the conclusion that only by lopping the head off the government machine in the American fashion can an incoming government bring about a measure of effective legislative change.

The second principal theme is the failure of the Labour government to bring about this change. Having no clear notion of priorities, despite thirteen years of Conservative rule, the government disillusioned its supporters more than it frightened its political opponents. Most ministers were grossly overworked and unable to devote more than nominal attention to policies outside their own departments. Cabinet committees, supervised by civil servants, made many of the crucial decisions, while some members of the Cabinet were left ill-informed on wide areas of government policy. Treasury ministers became Treasury prisoners, defense ministers spokesmen for the armed forces. Even the self-aware, observant Crossman felt the waters of personal comfort and public recognition closing over his head. Hence it is no wonder that after nine months of Labour government he should note in one crucial committee of senior ministers "a fantastically rigid adherence to basic programmes we had inherited from the Tory Government. Tory priorities were prevailing over our own socialist loyalties" (p. 281).

There is much else to savor in this book, not least Crossman's comments on people and issues during his many ministerial and party-political trips throughout Britain. The principal flaw, inescapable in a diary, is his failure to consider issues thoroughly or reflectively.

*The Diaries of a Cabinet Minister* will disillusion believers in a parliamentary road to socialism. But for those who would understand either Labour politics or the government of Britain it is both compulsive and compulsory reading.

DAVID RUBINSTEIN  
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ANTHONY SLAVEN. *The Development of the West of Scotland: 1750-1960*. (Regional History of the British Isles.) Boston: Routledge and Kegan Paul. 1975. Pp. xvi, 272. \$20.25.

This inaugural volume in a new series on the Re-

gional History of the British Isles is, according to the editor, aimed at "the widest possible readership consistent with this standard of scholarship." The statement is vague, but, in the present instance at least, the standard is high, and one hopes that the readership is not inversely correlated thereto. A pedantic reviewer might well claim that both "Regional History" and "Development" in Anthony Slaven's title are misleading or at least should be severely qualified by adding the terms economic and social. Indeed, even the social content is rather limited, most of the volume being taken up with industrial history; hence the limitations of scope restrict somewhat the author's capacity to bring out the distinctive regional characteristics of his area. Nevertheless, Slaven is in complete command of the subject matter within his chosen field, and one suspects that he has drawn substantially on his own research as well as the latest publications by other scholars.

The type is small and the contents are densely packed; consequently one suspects that the general reader will find the volume rather formidable. While scholars will be appreciative and hopeful that later volumes in the series will maintain this standard, they will inevitably complain about the absence of footnotes and the limited bibliography and index. Moreover, the maps are not easy to read. The organization of material is fairly conventional and each of the two main periods—the division is at 1870—is accorded its due share of space.

A. W. COATS

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T. W. MOODY *et al.*, editors. *A New History of Ireland. Volume 3, Early Modern Period, 1534–1691*. New York: Oxford University Press. 1976. Pp. lxiii, 736. \$29.95.

This imposing volume is the first to appear in the long-awaited *New History of Ireland*, a nine-volume enterprise under the auspices of the Royal Irish Academy. Its publication after a decade of painstaking preparation constitutes an event of importance and an augury of things to come.

The format is essentially that of Lord Acton's *Modern History*. Sixteen contributors have written twenty-three chapters. Roughly half of the chapters are chronological narrative, and the other half topical. The four major contributors to the chronological sections are: G. A. Hayes-McCoy, for the sixteenth century; and Aidan Clarke, Patrick J. Corish, and J. G. Simms for the early, middle, and late seventeenth century, respectively. The chronological chapters, as a whole, provide for this "age of disruption" (T. W. Moody's phrase) a narrative which is definitive, judicious, and objective. Despite the assorted backgrounds of the contributors,

there is virtually no evidence of the sectarian ax-grinding which plagued an earlier age of Irish scholarship. The perspective attained is far broader than that of Richard Bagwell, whose six volumes (*Ireland under the Tudors*, 3 volumes, [1885–90]; *Ireland under the Stuarts*, 3 volumes [1909–16]) heretofore comprised the principal account of the period. Scholars may discover some minor inaccuracies and oversights or differ with particular interpretations and assessments, but many decades will pass before the broad outlines of this authoritative treatment require correction.

The topical chapters tend to be more engaging, and also more provocative. Scene-setting chapters on "Ireland in 1534" (D. B. Quinn and K. W. Nicholls), "Land and People, c. 1600" (R. A. Butlin), and "Land and People, c. 1685" (J. H. Andrews) offer glimpses of the unchanging, or slowly changing, patterns of life in early modern Ireland. Aidan Clarke and L. M. Cullen assess the Irish economy; the former from 1600 to 1660, the latter from 1660 to 1690. Three chapters on "The Irish Language" (Brian Ó Cuív), "The Development of the English Language" (Alan Bliss), and "Irish Literature in Latin" (Benignus Millet) prove central to the project by illustrating the virtually tripartite nature of Irish culture in this period. Somewhat more peripheral is an interesting, short chapter on "The Irish Coinage" (Michael Dolley). Finally, the intriguing subject of "The Irish Abroad in the Age of the Counter-Reformation" (John J. Silke) receives a conscientious summary which whets the appetite.

Volume 3 of *A New History of Ireland* is enriched by twenty-one maps, fourteen illustrations, and a sixty-page bibliography, as well as numerous footnotes, useful introductions to both the text and the bibliography, a list of contributors, and an impressively thorough index. The work is printed and bound to the still exacting (though now very expensive) standards of the Clarendon Press and will surely take a place as one of the major collective accomplishments of European historical scholarship in the second half of this century. Additional material of relevance will be incorporated into volumes eight and nine which are envisaged as an appendix to the first seven volumes of text. That will be welcome, as will the other volumes of text, but even standing alone *Early Modern Ireland, 1534–1691* is a work entire, shedding needed light and knowledge in a long-obscure corner of the past.

KARL S. BOTTIGHEIMER

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RICHARD NED LEBOW. *White Britain and Black Ireland: The Influence of Stereotypes on Colonial Policy*.



Philadelphia: Institute for the Study of Human Issues. 1976. Pp. 152. \$10.95.

Richard Ned Lebow is interested in Irish history and in how and why the colonial empires of modern democratic states have collapsed. In this brief study he has combined both of these interests by using nineteenth-century Irish history as a source for generalizations about the decline and fall of modern colonial empires.

The author briefly surveys the nineteenth-century Anglo-Irish political background and questions why successive British governments generally pursued policies in Ireland that were counter-productive. He finds some answers to this question in the popular images of Irishmen accepted by British policy-makers and voters. According to Lebow, Irishmen were stereotyped as lazy, superstitious, accustomed to poverty, violence-prone, and incapable of governing themselves. The perceptions of the general public and policy-makers were so distorted by these stereotypes that British officials were unable to respond to the demands of Irish protest organizations. These stereotypes formed what Lebow calls a perceptual prison for British officials and prevented them from understanding the nature of Irish protest movements or correctly assessing the amount of popular support behind them. Lebow concludes that the stereotypes that justified a continuing British presence in Ireland contributed in the end to the collapse of British power there.

This book is not traditional historical scholarship. It makes extensive use of social and behavioral science concepts and generalizes the British experience in Ireland into an interpretation of the decline and fall of colonial empires throughout Asia and Africa. One could wish that the author had applied his in-depth analysis to periods other than the 1840s. The nature of the Irish question changed through time. Comparison of the responses of the British government to perceptions of Irish question in the 1840s with those of the 1880s and the years after 1900 would have been instructive. Yet, Lebow wrote the kind of book that he wanted to write, and all students of the Irish question will be rewarded by reading it.

ROBERT F. BURNS  
*University of Notre Dame*

CATHERINE SANTSCHI. *Les évêques de Lausanne et leurs historiens des origines au XVIII<sup>e</sup> siècle: Érudition et société.* (Société d'histoire de la Suisse romande: Mémoires et documents. Series 3, number 11.) Lausanne: Société d'histoire de la Suisse romande. 1975. Pp. xiv, 453.

From the eleventh to the eighteenth centuries local scholars compiled lists of the bishops of Lausanne.

They normally began with the bishop St. Protasius in 652. Some of them continued until the Protestant expulsion of the ecclesiastical establishment in 1536. A few even listed the bishops residing in Fribourg after 1536 who continued to claim authority over Lausanne. The lists were compiled for a number of purposes. In the beginning they served to establish a calendar of rituals designed to honor past leaders of the local church. They were later used to establish claims of dependence of the diocese upon the papacy, which were designed to ward off efforts at control by secular governments. They were finally used as a chronological framework for political histories of the area.

Catherine Santschi has devoted this monograph to an intensive study of these lists and the histories built upon them. Many of these works were never published and are now widely scattered. She has devoted a good deal of energy and impressive technical skills to the tasks of locating and interpreting them. She analyzes them in four chronological categories: medieval chronicles, humanist literary histories, seventeenth-century scholarly studies, and the eighteenth-century synthesis of Abraham Ruchat. Santschi is not primarily interested in the objective accuracy of these works, nor does she seek to evaluate in detail the philosophical frameworks of their authors. She concentrates primarily on the forms and method used in composing them and has some interesting things to say about the polemical intentions of some of their authors. Each group of works is carefully set in the historiographical context of its time.

This book will obviously be an extremely useful research tool for any historian of the Lausanne area. It may also prove interesting to others as a case study of the methods and intentions of some local ecclesiastical historians.

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CHRISTIAN HUETZ DE LEMPS. *Géographie du commerce de Bordeaux à la fin du règne de Louis XIV.* (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 49.) Paris: Mouton. 1975. Pp. 661. 190 fr.

This book attempts to merge the methods of geography and history to supply material for further work and to demonstrate the nature of commercial life in Bordeaux at the start of the great century of its history. No analysis of the pattern or volume of trade is promised and little is offered. Christian Huetz de Lemps organizes his data first by exports and imports, then by the product, then by the region of origin or destination, and finally by date. In each case, volume interests him much more



than value. This division makes the work clumsy. At the end, with a nod to social history, there is a lengthy section about life on board ship and life in port.

The sources for this lengthy list of goods and their destinations include correspondence of *intendants* of Guienne, reports of the Bordeaux Chamber of Commerce, correspondence of merchants, but, most important, the unique records of the Admiralty of Guienne. From 1698 to 1716, with a few breaks, the toll collectors left surviving records for over 36,000 ships, including their names, the names of the captains, ships' tonnages, ports of destination, size of crews, and qualitative and quantitative information on cargoes. The find is so good that it is almost as if the only purpose of the book is to report it.

The data show the limited influence of the French government on the trade of Bordeaux. The volume of wine produced in any given year was more essential in determining the volume of goods shipped. Wine made up about seventy percent of exports by volume from Bordeaux through most years of the first two decades of the eighteenth century. The import of goods, especially sugar, from the French Caribbean islands rose sharply after the end of the War of the Spanish Succession. Bordeaux became a center of re-export of tropical goods to northern Europe as London and Amsterdam had been earlier. The war had surprisingly little effect on the trade of Bordeaux, a fact Huetz de Lemps attributes to the system of passports given to enemy, and especially Dutch, shipping from 1704 to 1710. The poor state of trade from the Garonne in 1702–04 is blamed on the prohibition of enemy shipping. Since the later interdict of 1710 had a much milder effect, the more likely explanation is the agreement between the Netherlands and England not to trade with France. The Dutch refused to renew that agreement when it ran out in 1704 because it had been a disaster for the commerce of Holland. The bibliography, though extensive, lacks any works in Dutch or German, so Huetz de Lemps has missed the other side of Bordeaux's most important trading relationship.

There is too much detail. A statement is often followed by example after example, material that belongs more properly in appendixes or tables. There is also a good deal of repetition. It is strange that a book of over 600 pages requires only a four-page conclusion. The book, however, is, as promised, filled with a mass of novel material. At least it is now certain that in one year Bordeaux imported more than 7.5 million garlicks.

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JACQUES AMAN. *Les officiers bleus dans la marine française au XVIII<sup>e</sup> siècle*. Preface by MICHEL MOLLAT DU JOURDIN. (Centre de Recherches d'Histoire et de Philologie de la IV<sup>e</sup> Section de l'École Pratique des Hautes Études. Volume 5, Hautes études médiévales et modernes, number 25.) Geneva: Librairie Droz. 1976. Pp. xiii, 201.

In this model of painstaking research, the product of many years' work in the marine archives, Jacques Aman traces the evolution of a naval institution through its personnel dossiers and, in so doing, corrects a number of errors and mistaken judgments perpetuated by historians.

Who were the *officiers bleus*? A large part of Aman's study focuses on defining this amorphous group. Contrary to legend, their name did not arise from the blue uniform they wore in contrast to the "reds," the regular aristocratic naval officers. The term *bleu* antedates the uniform and refers to the temporary employ of one not integrated into an administrative hierarchy. The "blues" consisted of merchant captains or aspiring merchant captains, colonial officials in the *Compagnie des Indes*, and petty officers in the royal marine, such as pilots, who volunteered for service as subaltern officers in the navy in times of war. They rarely served in active combat, but instead fulfilled support functions—as convoys of merchant ships, in the transport of troops and supplies, in coastal defense, or as privateers in the *guerre de course*.

Traditional historiography claims that there was fierce rivalry between the "reds" and the "blues" based upon aristocratic prejudice. While admitting in principle a certain natural hostility based primarily on a professional esprit de corps against outsiders, Aman seriously questions the extent of the so-called "noble reaction." Written proof of this hostility is largely lacking; indeed, documentary evidence furnishes numerous examples of praise of the "blues" by aristocratic officers—perhaps because a small percentage of the "blues" were themselves of noble origin and had entered naval service in this way because they could not afford the expense of entrance into the select officer corps.

Aman rejects the traditional view that the "blues" took over the staffing of the navy as a result of the emigration of noble officers during the Revolution. Instead, he suggests that eighteenth-century secretaries of the navy deliberately enlarged the role of the "blues" by limiting the number of aristocratic cadets who could become naval officers. Then the "blues" underwent a transformation; the most talented evolved into a new body, the *auxiliaires*, who were paid half of their salary in times of peace and who constituted a reserve pool for times of war.

Narrow in scope, judicious in the use of evidence, Aman proves once again that all careful research must go back to the documents themselves, rather than accept what traditional history tells us is true.

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JEAN-PIERRE FILIPPINI *et al.*, editors. *Dossiers sur le commerce français en Méditerranée orientale au XVIII<sup>e</sup> siècle*. Preface by ROBERT BESNIER and NICOLAS SVOIRONOS. (Travaux et recherches de l'Université de droit, d'économie et de sciences sociales de Paris, Série sciences historiques, number 10.) Paris: Presses Universitaires de France. 1976. Pp. 251.

Three essays by three different authors make up four-fifths of this book. They deal with the coffee trade, French relations with Crete, and the activities of two men named Maurepas and Villeneuve. The port of Marseilles is an important link between these disparate pieces. Each of the three carries a theme beyond the works of Marseilles' great older historians—Paul Masson, Gaston Rambert, and their collaborators. The preface calls the three authors an *équipe*, perhaps because all have worked with the same professors, or because all have tapped the consular correspondence and are engaged in producing works on French Mediterranean trade in the eighteenth century.

The first essay, by Claude Roure, traces the implementation of classic French commercial policy ("*dirigisme*") in Mediterranean trade during the embassy of the Marquis de Villeneuve at the Porte (1728–41); her meticulous scholarship in this article augurs well for her thesis on the same subject. The essay by Louis Meignen, a sketch of the French coffee trade with the Levant, is also a careful study. Meignen shows how the merchants of Marseilles reversed their role in this period; having been importers of coffee from the Levant in the 1720s, they became by the 1780s major importers of American-produced coffee, which they then re-exported to the Levant and to European markets. Daniel Sabatier, also using the consular archives, offers an assessment of trade between Crete and Marseilles after the expulsion of the Venetians from that island in 1669 until the mid-eighteenth century, a period during which a flourishing traffic in olive oil developed after the destruction of the French groves in Provence (1709). Sabatier's well-balanced paper includes two dozen charts and graphs.

Two shorter pieces are also included: J.-P. Filippini's article draws attention to the thesis he is preparing on *la nation française* at the port of Leghorn in the period. George Stéphanidès, who is

preparing a guide for Greek studies in French archives, supplies an *orientation sur les sources* listing archives, inventories, and recent articles.

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ELIZABETH FOX-GENOVESE. *The Origins of Physiocracy: Economic Revolution and Social Order in Eighteenth-Century France*. Ithaca: Cornell University Press. 1976. Pp. 325. \$15.00.

Elizabeth Fox-Genovese has given us a major contribution to a field until recently very much neglected. In her preface she speaks of "the excitement of close textual and theoretical analysis . . ." (p. 30), and she manages to convey that excitement to her readers. For those who have often despaired over the complexities of Quesnay's articles in the *Encyclopédie* and for those to whom the "zigzag" or zig-zag of the *Tableau économique* has appeared as a mysterious decorative design the meaning of which eluded them, the author provides closely reasoned and clear explanations. Among the best of her nine chapters is without doubt chapter 5, in which she unravels, in the best tradition of the *explication de texte*, the extraordinary dialogue between Quesnay and Mirabeau, the founders of the "*parti*." Only such microscopic examination of their interrelated thinking can really lead to a clarification of the *Tableau* with which this study ends.

Fox-Genovese is quite right in reminding us that "Enlightenment . . . itself cannot be fully understood apart from physiocracy. . . ." (p. 306). Her study therefore has not only focused new light on one aspect of the eighteenth-century movement of ideas, it has illuminated the movement as a whole. If she is to be faulted it is for sometimes using language carelessly and for blurring some distinctions. *Grains*, *Fermiers*, and *Evidence* by Quesnay are included in the *Encyclopédie*. *Hommes*, though intended for it, was not used by the editors. The term "bourgeois," besides the meanings suggested by Fox-Genovese, also had a juridical reality which we must keep in mind. Mirabeau may have been using the term "advantages" euphemistically (p. 154), but in the context he may also have been using it in its very precise legal sense of the right to leave by will above and beyond what is due under the custom of the region.

The author reminds us of the new meanings the Physiocrats gave to old words. She is right when she stresses that the *Economistes* "contribute to a shift in intellectual paradigms." They also helped to forge a new vocabulary and created new words, not least among them that ubiquitous word civilization which we use so freely. Neither Voltaire

nor Rousseau speaks of civilization, but it appears several times in Mirabeau's "*L'ami des Hommes*." Perhaps in her next book Fox-Genovese will develop further the themes she touches only lightly in her present, excellent study.

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FRANÇOIS-XAVIER EMMANUELLI. *Pouvoir royal et vie régionale en Provence au déclin de la monarchie: Psychologie, pratiques administratives, défrancisation de l'Intendance d'Aix, 1745-90*. 2 vols. Lille: Service de reproduction des thèses, Université de Lille. 1974. Pp. lxxxii, 1-414; 415-946.

François-Xavier Emmanuelli's *thèse* (Université d'Aix-en-Provence, 1971) follows the *Annales* pattern: an attempt to write total history, theoretical constructs and methodologies drawn from the social sciences, statistical analysis of evidence, and a quest for patterns of human collectivities. In a way, it fits the pattern so neatly that there is reason to suspect that the pattern has become a stereotype. Everything must begin with the "Land," and all information somehow becomes relevant, even information about how to treat sheep suffering from diarrhea. In part, the problems of Emmanuelli's study stem from its form. It is a reproduction of typescript and is too unpolished. There are too many errors, graphs not clearly drawn, and structural deficiencies. The author needed, and deserved, a good editor, because his work deserves serious attention.

What Emmanuelli has done is to apply the *Annales* approach to institutional and administrative history. He has attempted to write the total history of the Intendancy of Provence from the 1740s to the Revolution. He knows where to find his materials, and he has read a massive amount of archival and printed sources. Although he admits gaps in his evidence, he draws from it intelligent inferences and asks intelligent questions about the transition from the Old Regime to the Revolution.

The Intendancy of Provence during the period studied was dominated by one person: Charles Jean-Baptiste des Gallois de La Tour. Emmanuelli has read La Tour's correspondence, and he describes the principles upon which the intendant based his administration. But he goes beyond principles to context and action; he analyzes the sources of La Tour's income and describes in detail his family ties and personal relations with members of various institutions (civic, military, judicial, and economic), the personnel and expenses of his bureau, and even the contents of his library.

The basis of Emmanuelli's study, however, is a statistical analysis of the 2,239 *laisses* and registers that constitute the C Series (Intendance) of Pro-

vence for the years 1745-90. From these sources he calculates the *affaires* in which the intendant intervened—19,825 cases or actions for a period of forty-five years. These interventions involved an extraordinary variety of functions, structures, and issues: justice, finance, community and military concerns, social relations, public works, education, *mendicité*, and censorship. That is enough to give a flavor of this extraordinary study.

From this statistical analysis Emmanuelli draws conclusions about the nature and function of the Intendancy of Provence and its relationship to both the central power and the province. La Tour was neither a king's man nor a man of the province. He was, rather, "*l'homme du Contrôleur général*," a fiscal agent. He was a moderate mercantilist, a moderate physiocrat, and moderately enlightened. He was not an innovator. Above all, he represented the viewpoint of the notables and the property-owners. As a jurist (he was also First President of the Parlement), he was most concerned with maintaining order, and he believed that the only way to carry out his charge was to maintain both the *droits* of the king and those of the provincials. "*L'administrateur royal se posait en juge entre le Souverain et ses sujets!*" (II: 903), and more often than not he judged in favor of the provincial traditions rather than in line with royal and ministerial directives.

Emmanuelli knows the literature of the French intendants, and he admits that La Tour may not have been representative. He also observes that his conclusions will remain tentative until we have systematic studies of the *Contrôleurs généraux*. But he provokes, and he makes us reflect on the clichés about the Old Regime, especially about absolutism, centralized monarchy, bureaucratic centralization, ministerial despotism, and the crisis of the Old Regime. Institutional and administrative history may currently be in a relative state of neglect, but it still has much to recommend it when it is practiced by a historian like Emmanuelli.

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MAURICE GARDEN. *Lyon et les lyonnais au XVIII<sup>e</sup> siècle*. Paris: Flammarion. 1975. Pp. 374.

MARCEL COURDURIÉ. *La dette des collectivités publiques de Marseille au XVIII<sup>e</sup> siècle: Du débat sur le prêt à intérêt au financement par l'emprunt*. Preface by PIERRE CHAUNU. Marseilles: Institute Historique de Provence. 1974. Pp. 294.

Maurice Garden's *Lyon et les Lyonnais au XVIII<sup>e</sup> siècle* is a social and demographic study of one of the principal provincial cities during the last decades of the *ancien régime*. *La dette des collectivités pub-*

*liques de Marseille au XVIII<sup>e</sup> siècle* by Marcel Courdurié is a fiscal study of the *collectivités* and, more particularly, an examination of lenders and lending practices.

Despite the fact that it has been stripped of footnotes and bibliography, *Lyon* represents an interesting slice of urban life. Demographic graphs chart baptisms, marriages, and burials for the eighteenth century. Socioeconomic data in graph form illustrate the income of silk workers, compare the incomes of journeymen with those of master silk workers, and chart the price of wheat from 1700 to 1790. For primary sources, Garden has relied heavily on parish registers, tax records, and notarial minutes, with special emphasis on marriage contracts.

Lyons, whose eighteenth-century population numbered about 150,000, displayed the characteristics of a preindustrial urban center. Treated like a "minor" and continually reminded of its political, social, economic, and cultural inferiority to Paris, Lyons saw its wealthy and successful merchants and bankers being drained off to the capital.

In spite of appearances of demographic stability (and certainly aggregate population figures give this impression), Garden pictures a very fluid society. For example, in any given generation, the majority of the population had not been born in Lyons. An unusually large percentage of new-born children were sent to a wet nurse in the countryside. Numerically, if not socially, their places were taken by the numerous abandoned and orphaned children who were brought from the surrounding villages to the hospitals of Lyons. Garden postulates that the extraordinarily large number of infants sent away from home had an accelerating effect on Lyons's birth rate, which in turn meant that more and more children were being raised away from their parents and their *ville*.

Garden pictures Lyons as a *ville-tombeau*, but the drain on its population was more than offset by the arrival of men and women from the nearby countryside, who were attracted to the opportunities they perceived in Lyons. The consequences of immigration and emigration thus created the illusion of a stable population. The newcomers, however, found themselves systematically excluded from the choice occupations and neighborhoods. In Lyons, the classic *ville marchande et bourgeoise*, the gap between a wealthy minority and "la masse des travailleurs" increased in the eighteenth century, and the events of 1789 did nothing to change the domination of the bourgeoisie (p. 353).

Courdurié is primarily interested in the fiscal operation of the Marseilles *collectivités*, and his is financial history at its most detailed. Courdurié's extensive bibliography is a guide to numerous manuscript collections as well as the secondary

literature. Included also are thirty-four tables and sixteen graphs dealing with the indebtedness of the Marseilles institutions he discusses.

The first section of *Marseille* deals with the legal positions of lender and borrower, a discussion that takes the author back well before 1700. He examines ecclesiastical authorities, which originally had as their goal the protection of the poor, relevant civil laws in France, and, finally, the evolution of the attitude of the Marseillais themselves. "Thus the Marseillais had chosen between God and Mammon. They lent without troubling their consciences not only to the *collectivités*, such as the Chamber of Commerce and the Communauté, which had some resources—taxes or dues—but also to those like the Mont-de-Piété or the hospitals, which they knew did not possess sufficient revenues to finance their assistance to the poor and sick" (p. 91).

The second section deals with the three types of *collectivités* of Marseilles: the Community itself and two of its agencies that were organized to provide grain in time of scarcity and to guard the public health from such devastations as epidemics and plagues; the Chamber of Commerce and the *corps des courtiers royaux*; and the Hôtel-Dieu, the General Hospital, and the Mont-de-Piété, all founded before the eighteenth century to provide municipal assistance, particularly to the poor. Courdurié is more interested in the manner of financing and the origin of investments than he is in how the agencies functioned or whom they aided. For the period 1700–93 he has recorded 12,857 loans to the *collectivités* (from 5,914 persons) that were worth more than 66,000,000 livres. Although the investors came from most social levels, one interesting conclusion is that the propertied class almost always lent money as a form of "capitalistic" investment, whereas the lower classes invested, especially in the third category above, as we might in a savings bank, "assuring a modest revenue, so precious in difficult times—times of unemployment, illness, accident or old age" (p. 247).

These two studies will be of interest to all students of the *ancien régime* and are valuable additions to the growing literature of urban studies in the eighteenth century.

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JEAN-PIERRE GROSS. *Saint-Just, sa politique et ses missions*. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, number 31.) Paris: Bibliothèque Nationale, 1976. Pp. 570. 115 fr.

One of the most fascinating figures of the French Revolution was Louis Antoine de Saint-Just, that austere and enigmatic member of the Committee



of Public Safety made familiar to Americans by E. N. Curtis, *Saint-Just, Colleague of Robespierre* (1935), and R. R. Palmer's ever-popular *Twelve Who Ruled* (1941). Those interested in Saint-Just, the Terror, and the revolutionary armed forces will welcome this important new book by Jean-Pierre Gross.

It is not a biography. Nothing is said of Saint-Just's life before 1793, and his activities in the great Committee are surveyed only briefly. Within the Committee Saint-Just was preoccupied with army affairs, a natural predilection for the son of a cavalry officer. Gross acknowledges the influence of Robespierre over his younger colleague in political matters and their collaboration during the proscription of the "factions," but shows that the two men normally worked independently.

The book is essentially a comprehensive analysis of Saint-Just's missions to Alsace in the fall of 1793 and to the Army of the North in 1794. Gross bases his conclusions on exhaustive archival research in Paris and the departments. Extensive information is given on the small group of "friends" who surrounded Saint-Just, and upon whom he relied heavily. Special attention is devoted to the military courts that he established and to the incidence of the Terror. The picture that emerges is not of a bloodthirsty extremist. For Saint-Just, the Terror was a temporary, strategic action necessitated by the defense of the frontiers.

Emphasis is also placed on Saint-Just's social policies and his efforts to propagate revolutionary spirit in Alsace. Much of the money which was raised by a forced loan on the rich of Strasbourg was ultimately given to the poor. As for Saint-Just's celebrated decrees ordering the establishment of French schools and the suppression of native dress, Gross argues persuasively that they were not instances of "linguistic Terror" but instead well-intentioned attempts to assimilate Alsace into the Republic.

To Gross, Saint-Just was a man of action. He mobilized human and material resources for the army and deserves considerable credit for French victories in Alsace and at Fleurus on June 26, 1794. His inexperience with clubs and the Parisian crowds helps to explain his mysterious indecisiveness on 9 Thermidor.

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MAX FAJN. *The Journal des hommes libres de tous les pays, 1792-1800*. (Studies in the social sciences, number 20.) The Hague: Mouton. 1975. Pp. 194.

This account of the *Journal des hommes libres de tous les pays* and its continuations is a well-documented, though narrowly focused, history of the Jacobin newspaper from its establishment in November

1792 through its suppression in September 1800. Max Fajn's method provides for a brief chronological survey of the newspaper for each of eight distinct periods, followed by circulation estimates, occupational and geographic distribution of readership, identification of contributors, and content analysis. Though those responsible for the *Journal*, particularly its long-suffering publisher René Vatar, rarely emerge from the shadows, Fajn convincingly demonstrates that the newspaper itself "was not an updated version of Hébert's *Père Duchesne*, nor was it preoccupied with decrying the crimes of the royalists and the threat of counter-revolution," as has been maintained by Vandal and Madelin. Its editors disliked the machinations of the Directory and criticized the regime's periodic oscillations to the right. But, according to Fajn, the *Journal* supported constitutionalism and almost consistently urged republicans to support the government as insurance against a royalist counter-revolution. Unfortunately the Directors equated criticism with disloyalty, and Bonaparte simply had no need for an opinionated Jacobin newspaper once the Consulate became entrenched.

That the revolutionary regimes were unable to come to terms with an independent press is illustrated by the dual practice of harassment and subsidization. Fajn shows this clearly. His account of the *Journal's* position during the trial of the Babouvists cuts away traditional oversimplifications. On the other hand, Fajn's data on press runs and circulation estimates seem insufficient, and the letters to the editor column is problematic as a gauge of the profession of readers. Though the volume is marred by grammatical slips, spelling errors, clumsy translations, and faulty proofreading, it confirms the fact that many more limited studies of individual newspapers are needed before we can hope to understand the subtle interrelationships between the press and the course of the Revolution.

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CLAUDE LANGLOIS. *Un diocèse breton au début du XIX<sup>e</sup> siècle: Le diocèse de Vannes au XIX<sup>e</sup> siècle, 1800-1830*. (Institut Armoricaire de Recherches Historiques de Rennes.) Rennes: Université de Haute-Bretagne. 1974. Pp. 629.

Inspired and guided by the seminal studies of Messieurs les abbés Mahuas and Cariou, Claude Langlois has presented French ecclesiastical historians with the definitive work on the diocese of Vannes. Focusing his attention on the three decades following the enactment of the Napoleonic Concordat (and Organic Articles), Langlois has fathomed the unique religiosity of the Morbihan



and in so doing successfully disputed Marx's invidious characterization of this remarkable region as *marais pontins*.

His exhaustive efforts to probe the persistently archaic clericalism of Vannes reflect the demanding methodology of Descartes' four inflexible rules of logic. With the most painstaking meticulousness Langlois analyzes the multi-faceted impact of the Revolution of 1789; the forms and structure of post-Reformation Breton Catholicism; the real ecclesiastical leaders in Vannes and their relationships to subordinates, rivals, and imperial officials; and the adverse living conditions in the Morbihan during the early nineteenth century.

Encompassed in these investigations is a massive study of catechisms, manuals on theology, spirituality and piety, liturgical treatises, and seminary curricula. In addition, the quality, quantity, and subject matter of pastoral preaching in Vannes are subjected to intensive analysis. Indicative of the degree to which Langlois has scrutinized sermons delivered by Breton clergy during the period under consideration is his graph diagramming the frequency with which the priests had recourse to such themes as salvation, death, mercy, heaven, hell, mortal sin, judgment, and drunkenness (p. 575).

Langlois also has tried to measure the enthusiasm and religious fervor of the faithful who lived in the shadow of the *chouannerie*. He compares urban and rural Catholicism from this standpoint. Epicenters of popular religion are also identified, and the spread of catechetical teachings within these areas is contrasted with that of localities tending to exhibit concentrations of religious elitism.

At times Langlois seems to express his thoughts as a stream of consciousness, in which both cumbersome sentences and rhetorical flourishes abound. He also displays a tendency toward pedantry. Such minor deficiencies, however, are compensated for by helpful summaries which appear at the end of most chapters.

In any event, readers of Langlois's opus will find a striking analysis of a most remarkable enclave of dedicated Roman Catholicism. Neither the chaos nor adversity of the First Empire and Restoration monarchy seriously eroded religious stability in the diocese of Vannes. After 1807 ordinations increased steadily, the number of missions expanded, religious retreats could be made without danger, each parish had a rector, every village possessed at least one church, new orders were founded, while established ones attracted more tertiaries than ever before.

Langlois attributes the successes of Vendérian clericalism to multiple causation. Its leadership was charismatic and capable of mediating between rural and urban Catholics as well as the Church's patricians and plebeians. All members of the clerical hierarchy dutifully obeyed authority, the dis-

pensers of which in turn granted them some freedom to express their individualities. The establishment of such a *modus operandi* encouraged most ecclesiastics to cooperate with one another, express docility toward superiors, conform to the highest levels of morality, and work zealously for the perpetuation of their faith.

Through the efforts of Langlois, it is now possible to understand *catholicisme vannetais*. Consensus, continuity, and cohesion created within it a capacity to express the fullest devotion to the facts and doctrine of the Church Universal.

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BERNARD H. MOSS. *The Origins of the French Labor Movement, 1830-1914: The Socialism of Skilled Workers*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xiii, 217. \$12.95.

This study merits the attention of all students of the industrial transition and of comparative labor movements. For the first time we have a coherent sociology of socialist ideology as it evolved through the formative phase of the working class in an important Western nation. The essential theme is that French socialism was rooted firmly in working-class experience. Penetrating beneath Marxist and liberal preconceptions, Bernard Moss demonstrates that the core ideology of the French labor movement throughout the nineteenth century was "trade socialism," a concept based on a federation of trade associations organized locally in producers' cooperatives within the political context of a democratic republic.

It grew out of the basic socioeconomic realities of industrializing France. Throughout the century skilled workers in handicraft or semi-mechanized industry remained the dominant element in the French labor force, both in terms of numbers and organizational capacity. While their sense of skill, of the dignity of work, and of intra-trade solidarity remained strong because the actual nature of their day-to-day work changed minimally, their independence, control over the job, relationship with employers, general conditions of work, and relative wage levels deteriorated under the impact of emerging capitalist structures in their trades. In short, they were artisans undergoing the process of proletarianization. Their initial response (from the 1830s to the 1870s) was an ideology of associationism—producers' cooperatives and direct control of the workplace—to be realized through the political instrument of the social republic, which would help them out-compete the capitalists. Louis Blanc's immense popularity in 1848 is thus explained. Proudhonian and Bonapartist petty-capitalist variations of this concept were not uncommon in the Second Empire, but the main thrust

remained clearly socialist, as manifested in the First International and especially in the ideology of the Paris Commune, which was neither Marxist nor Proudhonian, but associationist—now tied to a political concept of federated communes.

Moss argues that the advent of republican government in a labor-repressive atmosphere demonstrated to skilled workers the utopianism of their political perspective: once they achieved a degree of power, the small-propertied peasants and middle classes would turn their backs on trade socialism. As opportunist republicanism showed its true direction, skilled workers increasingly took a revolutionary and more collectivist stance that stressed proletarian political exclusivism. But federated trade socialism remained the root ideology: the shift was toward greater national coordination of the movement. Paul Brousse and Benoît Malon more perfectly reflected the outlook of the majority of skilled workers than did Jules Guesde, but as they slid into cooperation with the hated republicans in the later eighties, the skilled worker movement moved toward Allemanist *ouvrierisme* and ultimately toward the anti-politics of revolutionary syndicalism.

The comparative value of this work should be emphasized, but a caution registered. In many ways, it provides a model against which the evolution of working class ideology elsewhere can be gauged. It is doubtful, however, that trade socialism had the same degree of significance beyond the borders of France. Nowhere else were expectations of the social transformation to be ushered in by democratic republicanism so strong (the heritage of '93), and nowhere else was the rise of industrial capitalism so retentive of handwork and semi-artisanal industry. Moss's study needs a stronger groundwork in economic history, not only to illuminate the differences between the evolving structures of the working class in France and its more rapidly developing neighbors, but also to tighten his own analysis. In this context I am not convinced that the shift to greater collectivism and a revolutionary strategy in the 1880s was rooted only in a new assessment of political realities. After all, the authentic proletarian population was growing rapidly as mechanization spread, and more and more artisans were being proletarianized, particularly in semi-rural settings (one thinks of the works of Rolande Trempé, Joan Scott, and Harvey Smith), as a kind of second industrial revolution unfolded in France. Combined with the obvious capitalist bias of the Opportunist Republic, such forces surely contributed to the new orientation of skilled workers. Such comment, however, should take nothing away from a book of great interest to historians and militants alike.

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EDWARD C. CARTER II *et al.*, editors. *Enterprise and Entrepreneurs in Nineteenth- and Twentieth-Century France*. Baltimore: Johns Hopkins University Press. 1976. Pp. xx, 207. \$12.50.

The four papers in this book, which were read in a Johns Hopkins-Catholic University lecture series in 1973, vary widely in purpose and approach. In his contribution, Maurice Levy-Leboyer attacks the notion that French entrepreneurs followed "Malthusian strategies" in the nineteenth and twentieth centuries and thereby undercut the economic growth of France. He argues that "there were very few growth potentials that big business failed to exploit" (p. 127), and that if the French performance was less than optimal this resulted not from any irrational behavior on the entrepreneurs' part but from objective circumstances beyond their control. He thus exonerates the entrepreneurs by denying that entrepreneurship has been a crucial factor for French economic development.

By contrast, in his study of the textile dynasties of Roubaix-Tourcoing, David Landes again asserts that the quality of entrepreneurship is the crucial factor in French development. Arguing that local resource endowments were not sufficiently favorable to account for the expansion of the textile industries of Roubaix in the nineteenth century, he credits that expansion to the industrialists' dedication and zeal, which he traces, in turn, to their compulsive need to emulate and outshine their "betters" in nearby Lille.

While Levy-Leboyer and Landes thus rework familiar but important ground, Albert Boime and Charles Kindleberger venture into newer but more peripheral territory. Boime describes the role of businessmen as patrons of art and arbiters of taste and also explores the nature of entrepreneurial activity in the art "industry." Kindleberger assesses the influence of the French system of technical education on the personnel and practices of French business, concluding that that influence was somewhat negative but minor in the nineteenth century, more positive and more significant in the twentieth.

Obviously, no consistent or comprehensive interpretation of French entrepreneurship can be expected from four such distinct and divergent papers. Individually, however, each contributes to our understanding of the myriad forces acting upon entrepreneurs and the myriad ways entrepreneurs interact with their society. For that reason, their publication together in this handsome volume is welcome.

MICHAEL S. SMITH  
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C. STEWART DOTY. *From Cultural Rebellion to Counter-revolution: The Politics of Maurice Barrès*. Athens: Ohio University Press. 1976. Pp. 294. \$12.00.

Making good use of police reports and newspapers, C. Stewart Doty has reconstructed Barrès' political campaigns between 1889 and 1906, and throws new light on the relationship between the diverse socialist movements of the period, Boulangism, and the nationalist leagues. The way to understand Barrès' political thought, Doty tells us, is to focus not on his novels but on the local constituencies that elected him, on wards not words. As a young Boulangist in Nancy, Barrès' constituency was heavily working class, anti-Semitic and xenophobic (immigrant workers depressed wages). Boulangist strength in Paris also coincided with working-class districts. Left-wing Boulangists like Barrès, after they were elected to the Chamber of Deputies, consistently voted for working-class legislation and were accepted by socialists as bonafide allies. Socialism, fragmented and weak in the early 1890s, needed them, and they needed working-class support.

From these solid conclusions, however, to Doty's explanation of Barrès' own "socialism," and its organic link to fascism, is a quantum leap. Echoing the view of René Rémond, Eugen Weber, Zeev Sternhell and others that French fascism (and therefore earlier pre-fascism) was more Left than Right, Doty sees Barrès' shift to conservative nationalism after 1906 as a sharp break from his former socialist fascism. This view leads Doty to ignore his own evidence and to contradict himself. Doty claims that Barrès accepted a Marxian interpretation of history and the need to be "scientific," yet acknowledges that in his "socialist" novel of 1892, *L'Ennemi des lois*, Barrès attacked Marxist materialism, collectivism, and rationalism (what's left?) and preferred the Wagnerian Ludwig II of Bavaria to the "Jewish" Marx. Indeed, Doty later argues that it was Barrès' irrationalist "way of knowing" that was the "most serious flaw in his path to socialism." Doty first insists that Barrès' mingling with workers made him genuinely concerned with their plight, but later declares that Barrès could dispense with socialism so easily after 1906 because he had originally adopted it as "the fashionable thing to do . . . as an intellectual exercise." On page 208 Doty seems to take seriously Barrès' professed sympathy in 1903 for the "disinherited," only to comment on page 223 that in the campaign of 1910 Barrès, running this time in a conservative district, "could take off his pro-labor mask permanently." Similarly, we are told that Barrès was pre-fascist in 1903 because his nationalism was socially radical, even though in 1897 he had defended, according to Doty, "a cheap revolution of the 'spirit,' costing nothing in respectability, unlike the expensive socialist revolution of the material and real." Doty cannot have it both ways.

If the nature of Barrès' "socialism" is important (Hitler, too, was a defender of the workers—until

he came to power and busted their unions), the nature of his working-class support is equally so. Doty himself tells us that "only a minority [of Nancy workers] wanted to make strikes or radical politics. The rest were interested in mutual aid and consumer cooperative self-help. . . . Nancy socialism could easily accept Barrès' half a loaf over going hungry. . . . In the absence of a strong socialist or trade union movement, or in a region with only an embryonic working class, Barrès' brand of 'national socialism' could still make sense to large numbers of workers not yet preempted by real socialist politics" (p. 165). Despite earlier disclaimers, then, Doty does believe that some socialists are more socialist than others.

It is unfortunate that Doty whitewashes Barrès' anti-Semitism as "solely political and patriotic" and denies the racism of his anti-German novels. His is a revisionism with blinders.

This is not to deny the excellence of Doty's chapters on Boulangism *per se* and on the nationalist leagues a decade later. These chapters alone make the book a valuable addition to the study of modern French nationalism. The writing style is lucid and succinct.

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TONY JUDT. *La reconstruction du Parti socialiste, 1921-1926*. Preface by ANNIE KRIEGLER. (Travaux et recherches de science politique, number 39.) Paris: Presses de la fondation nationale des sciences politiques. 1976. Pp. 231.

In the last decade there has been a veritable explosion of literature concerning the French Socialist Party between the wars. This literature has concentrated on the 1930s and on Léon Blum. Unfortunately, the period between the Congress of Tours and 1934 has been greatly neglected.

This neglect is surprising. In 1921 the SFIO seemed close to extinction. The *vieille maison* had been deserted by most of its militants, and those who remained, the old right under Renaudel and the Longuet faction, had little in common. The former had fought against the Third International, the latter had been excluded from it. How could this divided rump survive? How was the party able to rebuild itself, so that by 1924 it was the second largest group in parliament? In what way did the experiences of these years permanently shape the party's behavior? These are the fundamental questions Tony Judt attempts to answer.

The book fills an important gap in our knowledge and is, moreover, a solidly researched monograph. The author has not restricted himself to parliamentary politics, but has examined in detail the situation in the provinces. It was there, after all, that the party would survive or fail. Judt organizes his work around four major topics: the

reconstruction of the party, the relationship of theory and practice, the party's position within international socialism and vis-à-vis the Communists, and its role within French politics. He stresses the effort of the SFIO to remain a "revolutionary" party while at the same time playing a role within the political system. It was its refusal to abjure its "marxist heritage"—a heritage that provided a common faith if not a basis for action—that distinguished the party from many of its social-democratic counterparts, but which also limited its freedom of action.

The major flaw of this book is its pedestrian quality. The author often gives us too many details when a summary would be in order. The wealth of documentation is not usually matched by incisive conclusions, and one feels all too frequently that the chapters are organized around a checklist rather than organically.

Considering the passionate quality of this period, one is surprised by how little excitement is conveyed in the prose. One reason perhaps is the author's inability to deal with personalities. It is not until the very end that he provides a few inadequate sketches of party leaders. Yet he recognizes that it is dangerous to envisage the role of a Blum or a Renaudel as similar in say, 1922 to what it was to be in 1932. The reader is constantly faced with a plethora of names of obscure local leaders, who are insufficiently identified. One loses sight of the fact that the party was rebuilt by real men.

There are some lacunae. The election of 1924 is constantly referred to but never discussed, the process by which large numbers of *quondam* Communists returned is barely treated, electoral sociology is avoided, as is a voting analysis of party congresses.

Despite its limitations, however, this work constitutes an important contribution to our understanding of a critical period in the history of the French Left.

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JOSÉ ANTONIO MARAVALL. *Estudios de historia del pensamiento español*. (Third Series.) Madrid: Ediciones Cultura Hispánica. 1975. Pp 407.

JOSÉ ANTONIO MARAVALL. *La cultura del barroco: Análisis de una estructura histórica*. (Letters and ideas, number 7.) Barcelona: Editorial Ariel. 1975. Pp. 536.

Probably no one has undertaken the cultural and intellectual history of seventeenth-century Spain with more active interest in its varied themes than José Antonio Maravall. The two volumes reviewed here are very different, but each represents a facet of the scholarly activity he has pursued since he completed his doctoral dissertation at the University of Madrid thirty years ago. His credentials for

undertaking the complex task of illuminating the seventeenth century are impressive.

The decline of Spanish greatness in the Baroque period was characterized by political decadence, economic weakness, military and international humiliation, famine, revolt, and the failure of the Habsburg dynasty. So state the general texts of the period. Paradoxically, Spanish cultural and intellectual life remained rich, varied, and productive, retaining much of the vigor of a happier time. Students of the period have long wrestled with the obvious problems and contradictions thus posed. Maravall's books add to our knowledge of the era, illustrating on the one hand specific themes and interests of writers and artists, and on the other developing a "structure" of the Spanish society in which they lived and worked.

*Estudios de historia del pensamiento español* is a collection of fourteen articles written between 1945 and 1972. Originally prepared for presentation at conferences or for professional journals, each has been edited and amplified for this publication. The topics covered include such subjects as the origins of empiricism in Spanish thought, Platonic influences in Gratian, the idea of liberty in the work of Fra Pedro Simón, and a study of painting and its relationship to reality in the work of Velázquez. Perhaps the most interesting and thoroughly developed essays are those which deal with political theory. In particular one should note the two essays on the impact of Machiavelli and "Machiavellianism" in Spain and another on the influence of Tacitus. Although these articles are uneven in quality and length, they make up an interesting series. One notes in passing that the volume has no index.

In *La cultura del barroco* Maravall's ambitious goal is a synthesis of all the strands of thought as well as the historical context from which they emerge. He attempts to view thought and culture in relation to the "structure" of Spanish society, its tensions, conflicts, and crises, whether economic, political, or social. It is a grand design that seeks to recreate the "Spanish mentality" that developed in an urbane, conservative society and to describe its world view. Further, Maravall hopes to dramatize for us how this elite conceived the role of humanity and used literary or artistic media to express their points of view. Certainly a prodigious undertaking, this masterful effort attempts to recreate for us the total civilization of the Spanish Baroque.

One is not convinced that the goal is achieved, but there is still a sense of satisfaction that the endeavor was undertaken, *La cultura del barroco* is recommended to every student of the period. One's appreciation of this volume makes the promised study of the sixteenth century by the same author welcome indeed.

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FRANCISCO MARTÍ GILABERT. *La abolición de la Inquisición en España*. (Instituto de Historia de la Iglesia, Universidad de Navarra.) Pamplona: Ediciones Universidad de Navarra. 1975. Pp. 358.

Francisco Martí Gilabert has built his book around a detailed examination of the stormy parliamentary debates and maneuvers that led to abolition of the Inquisition by the Cortes of Cádiz. His essential conclusions are that the liberals in the Cortes, by astute maneuver and debate, avoided a politically dangerous confrontation with the still popular tribunal. Rather, they raised the question of the compatibility of the Inquisition with the constitution of 1812. The dominantly liberal committee appointed to study the matter inevitably found the Inquisition and the constitution incompatible. In addition, they shrewdly proposed to draw the Inquisition's teeth by throwing conservative opinion a sop in the shape of revived episcopal jurisdiction over religious nonconformity through "tribunales protectores de la Fe," appointed by the bishops. These tribunals could not impose civil penalties, which were reserved for secular courts.

After prolonged debate, the Cortes abolished the Inquisition and set up the tribunals in February 1813. However, the decree of abolition encountered opposition (perhaps because triumphant liberals tried to force a sort of public humiliation on the clergy), and the Cortes moved legally to force acceptance. These actions were under way when the return of Ferdinand VII nullified the whole proceeding.

The extent of liberal or clerical sentiment in the Cortes of Cádiz has been the subject of lively controversy. Martí Gilabert finds much more Jansenist, liberal, and masonic influence than, for example, Ramón Solís does in his *El Cádiz de las Cortes*. Without embracing a conspiratorial interpretation, Martí argues that a liberal minority, moved by common ideas and a common concern for the future of Spain, manipulated rather than convinced the Cortes. Moreover, the author finds the abolition indefensible in canon law since, papal consent was never obtained.

Martí Gilabert's book seems to me to be a moderate and well-argued statement of a Catholic point of view, which, while willing to accept the end of the Inquisition, still harbors a certain distrust of the liberalism that brought it about.

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JOHN T. GRAHAM. *Donoso Cortés: Utopian Romanticist and Political Realist*. Columbia: University of Missouri Press. 1974. Pp. vii, 340. \$13.50.

Conservative thought in Spanish political history has lacked serious scholarship. Most of the studies

have been written by the conservatives and traditionalists themselves; a balanced view has therefore been absent. Except for the thrust of Javier Herrero's book (*Los orígenes del pensamiento reaccionario español* [1971]) for an earlier period, little else has been done. John T. Graham's book aims to fill in gaps and open new paths for peninsular political history.

The book is divided into eleven chapters which merge Donoso's life and thought. The author places the Spaniard in a European context, comparing and juxtaposing him with other counter-revolutionary politicians, particularly British, German, and Russian. In this sense he is accurate when he calls Donoso the Spanish Burke or the Latin de Maistre. He ends by thoroughly explaining the revival of Donoso during the Russian Revolution and in Franco's Spain. A selected bibliography and wide use of archival material further enhance the book.

In his political profile, Graham centers upon the famous *Essay on Catholicism, Liberalism and Socialism* (1851), an inflammatory appeal for Catholicism against what Donoso considered the hydra of anarchy devouring the world. He also brings forth minor, but no less important, speeches and articles Donoso wrote against materialism and the socialist revolution. Needless to say, Donoso's pen was sparked by the 1848 revolutions, which had echoes in Spanish political history (cf. C. E. Lida, *Anarquismo y revolución en la España del XIX*, [1973]).

Thus Donoso's evolution (or should we call it involution?) is from a romantic rebellious youth to a conservative advocate of dictatorship of Church and State, very much like Franco's Spain. In his study, the author draws from unpublished sources—dispatches, letters in the Ministry of Foreign Affairs, family archives, periodicals. The bibliography makes ample use of Spanish, French, and German studies, some of which are well known (Juretske, Sevilla Andrés, Suárez Verdager), as well as the comments of his contemporaries (Nicomedes Pastor Díaz, Gabino Tejado, Juan Valera), all moderates and conservatives and as afraid of "anarchism" and "chaos" after 1848 as Donoso was. A more balanced view could have been given had the author delved into the comments of his political opponents who advocated socialism and revolution—Fernando Garrido, Pi y Margall, among many others—particularly in those periods in which the more radical parties came into power, 1854–56, for example. This is of particular importance since, as Graham states, Donoso's influence waned after 1860. In this respect a wider use of parliamentary debates in the Cortes and the radical press should have been explored.

Graham's contribution is in putting Donoso in the perspective of European conservatism—Bismarck, Metternich, Louis Napoleon, Guizot,



Ranke, Schelling, Comte. However, a doubt arises in my mind as to his wide use of the term "utopia." It is a polisemic term which must be well defined. In a seminal study (*Das Geist der Utopie*, [1918]) Marc Bloch gave six definitions. He isolated six major expressions of utopia: medical, technical or alchemical, urbanistic or architectural, geographical, religious, and artistic. In short, utopias, according to Bloch, are aspirations, ideals of great social movements, well structured and coherent global visions of the world. Karl Mannheim, on the other hand (in *Ideology and Utopia*, [New York, 1967]), clearly established a difference between ideology and utopia: the first reinforces the existing social order and the latter is the protest of the oppressed to change that order. A coherent and clear definition is lacking in Graham's book, since from his analysis of Donoso Cortés' political thought, it is obvious that Donoso hoped to reinforce the social order in power in Spanish politics of the nineteenth century. Lamartine provided another good definition of utopia: "*Les utopies ne sont souvent que des vérités prématurées.*" In which category then does Donoso's utopia fall? Graham does not answer the question.

A last point in passing. The bibliography is lacking in studies of utopian socialism in Spain in the early part of the nineteenth century. Antonio Elorza, Clara E. Lida, and Maluquer de Motes have dealt with this subject at length and their conclusions would have further enhanced Donoso's political biography. The same could be said about Miguel Artola's contributions to nineteenth-century social and political history. Nevertheless, the author is to be commended for giving us a solid study which can be of use for future research into Spanish conservatism.

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PHILIPPE MOUREAUX. *Les préoccupations statistiques du gouvernement des Pays-Bas autrichiens et la dénombrement des industries dressé en 1764*. (Université Libre de Bruxelles, Faculté de Philosophie et Lettres, number 48.) Brussels: Éditions de l'Université de Bruxelles. 1971. Pp. 535. 775 fr. B.

In Brussels, late August 1749, the Frenchman Benoit-Marie Dupuy offered his services to the Austrian administration in the southern Netherlands. In view of the experience and knowledge accumulated in his prior post in the French tax administration in Brussels during the recent French occupation of the Netherlands, Dupuy was immediately appointed secretary to the government and placed in charge of inspecting and controlling the Austrian fiscal and financial administration in Belgium.

Philippe Moureaux devotes almost 200 pages to a narration of Dupuy's work from 1749 until his dismissal in 1757. Moureaux's analysis is a careful critical study of every major document of Dupuy's activities left in archives in Brussels, Vienna, Milan, and Paris. Thanks to meticulous scholarship, the book reveals much about the mentality and procedures of higher civil servants in the age of "enlightened despotism." The book also illustrates the emergence of statistical concerns in the higher echelons of an *ancien régime* administration.

The industrial survey of 1764, undertaken several years after Dupuy's departure, can be seen as a late outgrowth of his earlier work. The second part (250 pages) of Moureaux's book analyzes this relationship and critically compares the survey to other available statistics. Moureaux narrates the vicissitudes involved in this data collection and through comparisons with the 1738 (industrial) and 1755 (demographic) censuses gives a critical assessment of the accuracy and completeness of the 1764 numbers. He has published the latter in a separate book, *La statistique industrielle dans les Pays-Bas autrichiens à l'époque de Marie Thérèse* (1974).

Although the author passes a severe judgment on the 1764 document and sternly cautions against using it blindly, he warns scholars against a "hypercritical attitude, which is tantamount to intellectual capitulation" (p. 465). Rather than heeding his own advice, however, Moureaux has stopped short of reconciling any of the observed discrepancies between successive enumerations. Had he attempted this, scholars would now perhaps possess a rough approximation of output or employment in particular sectors or regions; as it stands, the reader is left to his own devices. It is perhaps disappointing that so much scholarship was applied to a document which evidently offers little substantive value to economic historians. But Moureaux must be congratulated for successfully carrying out his principal objective, for his book constitutes a very meticulous and interesting study in eighteenth-century administrative history.

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HARRY S. MAY. *The Tragedy of Erasmus: A Psycho-historic Approach*. St. Charles, Mo.: Piraeus Publishers. 1975. Pp. 180. \$4.95.

For generations Erasmus has been the victim of derogatory comment from those who resented either his failure to join Luther or his criticism of the Catholic Church, and who in neither case could explain Erasmus' attitude save by some grave defect of character. Now Harry S. May has repeated the time-worn process, but this time because of Erasmus' alleged hatred of Jews.

May seems strangely ignorant of the external essentials of Erasmus' life. His brief biographical sketch in the opening chapter is full of errors, some of them ludicrous. Yet he does not hesitate to probe deeply into his subject's subconscious, not deterred by lack of evidence or hampered by the rules of logic or of English usage. Although his research reveals no evidence in Erasmus' writings of anti-Semitism stronger than the prejudice against the non-Christian Jew normal in his age, May asserts that a corrosive hatred of Jews lay at the very core of the humanist's personality and his religious life. It was both cause and effect of the neuroses, the existence of which he verifies by citing several writers hostile to Erasmus.

With the aid of psychoanalytical clichés, May easily pinpoints the source of Erasmus' difficulties. He grew up without a father; hence "the Christ became his father substitute and surrogate who would suffer no unchristian deviationism" (p. 91). Again, "as his Christ-consciousness grew, so grew his anti-Jewishness in proportion. Both became his obsession and both made overwhelming demands on his life and his psychological well-being. The Jews unhinged him" (p. 12). In the penultimate chapter May expands his psychoanalytical speculation to include emotional and sexual disorders and a comparison of Erasmus with that other anti-Semite, Hitler. Among other similarities between the two, "Hitler suspected that his real grandfather was Jewish. Could it possibly be that Erasmus too had such fears and apprehensions?" (p. 139).

This preposterous book would not be worth a review in a learned journal were it not for the remote possibility that someone might take it seriously.

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J. C. H. BLOM. *De muiten op de Zeven Provinciën* [The Mutiny on *The Seven Provinces*]. Bussum: Unieboek B. V. 1975. Pp. 367. f 57.50.

In February 1933, part of the mixed Dutch and Indonesian crew of the Royal Netherlands warship *Seven Provinces* seized control of the vessel off the northwest tip of Sumatra. Six days later, after sailing southwards along the coast, the cruiser was attacked (perhaps mistakenly) by a Dutch military aircraft. A single bomb struck the ship killing twenty-three mutineers; the rest surrendered immediately.

This comprehensive monograph, originally a doctoral dissertation at Leiden University, maintains that pay cuts as well as generally poor morale in the navy rather than an active Communist cell among the sailors (as L. de Jong and others have argued) were responsible for the mutiny. J. C. H.

Blom is chiefly interested, though, in the impact the spectacular incident had upon the Dutch government and political parties. The former, supported by the officer corps and the predominantly "burgerlijk" (Calvinist, Catholic, and liberal) sociopolitical groups in the country, proceeded to root out social-democratic influences among naval unions and civil servants. Such "unreliable elements" threatened the loyalty of the armed forces and with it the nation's hold on its seemingly indispensable overseas possessions. In the general election of April 1933, the Socialist party (SDAP) lost two seats whereas the "law-and-order" Anti-Revolutionary party of the new prime minister, H. Colijn, gained two. Yet further to the right, the colonial-minded "Union for National Recovery"—founded only during the mutiny—suddenly won thirty-thousand votes and a seat in parliament. Even more spectacular was the rise of Anton Mussert's *Nationaal Socialistische Beweging* (one-thousand members in January 1933 22,000 a year later), attributable according to the Dutch Central Intelligence Service particularly to Hitler's coming to power in Germany and to the uprising on the "Seven Provinces."

The author nevertheless concludes that the mutiny was a relatively insignificant event: Dutch Nazism was a short-lived phenomenon (politically moribund by 1937) and the Social Democrats were only temporarily deflected from their course of self-integration into bourgeois Netherlands society. He perhaps underestimates one factor: an Indonesian nationalist led the mutineers.

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HARALD RUNBLOM and HANS NORMAN, editors. *From Sweden to America: A History of the Migration*. (A Collective Work of the Uppsala Migration Research Project.) Minneapolis: University of Minnesota Press. 1976. Pp. 391.

The comprehensive study at Uppsala University, 1962–76, of mass emigration from Sweden to America is recorded effectively in this volume. This impressive project, under the direction of Sten Carlsson, involved thirty postgraduate students, some undergraduates, and their faculty associates. Thirteen volumes and numerous articles have already been published. *From Sweden to America* is an interesting, informative, and scholarly final report.

This well-organized volume presents findings in vital aspects of mass emigration: sources, causes, chronology, and structure; historical and international aspects; functioning and organization of the transport area; demographic characteristics of the emigrant population; settlement patterns and receiving areas; and the assimilation process in North America. Research has also included oppo-

sition to emigration, emigrant agencies, emigrant policy, and remigration. A brief section describes emigration to Latin America.

The scope of this study involves more than one-million Swedish emigrants in the period 1860–1930 from a nation that had a population of only three and a half million inhabitants in the middle of the nineteenth century. The data used are for all of Sweden, although special studies include Hälsingland, Västernorrland, Östergötland, Dalsland, Småland, Öland, Örebro län, and the cities of Halmstad and Stockholm.

The interdisciplinary design includes models and theory from the social sciences. AID analysis is used in approaching certain parts of the vast and complicated mass of evidence, although caution is urged in testing it with other statistical data. "Push and pull factors," "cohort studies," "urban influence fields," and the "stock effect" (tradition of emigration) have received considerable attention. Several of the Uppsala researchers have questioned the use of econometric methods in emigration studies (p. 152). Although one author expressed the conviction that "the social-psychological aspects of migrational movements have been too much neglected within this research" (p. 75), the volume presents a reasonable balance of alternatives.

This cooperative project, which embraces the extensive and unique civil and ecclesiastical resources for Swedish emigration study, has produced interesting findings. Substantial evidence is presented to contradict the traditional outlook on Swedish emigration as primarily an agricultural and rural phenomenon, an attitude based largely on the twenty-two volume study by the Commission on Emigration (1907–13), under the leadership of Gustav Sundbärg. Compulsory military training as a factor is assigned a lesser role than the labor market. The influence of emigration agents and the transport sector was found to have had only a marginal effect. Moreover, except for a few highly publicized religious groups, the Uppsala researchers concluded that religious opinion was not a strong impetus to emigration. This latter conclusion is only modestly documented.

The research on remigration also provided new information. The detailed study for the period 1875 to 1930 showed a remigration rate of 18.2 percent. Largely a male phenomenon, it was "triggered" by American economic recessions. The American experience was not a stage in the process of urbanization and industrialization of Sweden because "about eighty percent of the investigated migrants moved in a consistent direction towards the parishes from which they had emigrated" (p. 320).

Since the emphasis is upon the Swedish background, the assimilation process in America receives brief attention. Understandable limitations

in the scope of the general project did not permit full recognition of the religious and cultural legacy of Swedish America. But two volumes of the Uppsala series in English are devoted almost exclusively to Swedish America: Ulf Beijbom, *Swedes in Chicago: A Demographic and Social Study of the 1846–1880 Immigration* (1971), and Sture Lindmark, *Swedish America, 1914–1932: Studies in Ethnicity with Emphasis on Illinois and Minnesota* (1971).

The editors identified above and Sune Åkerman, Berit Brattne, Sten Carlsson, Ann-Sofie Kälve-mark, and Lars-Göran Tedebrand, contributors, have greatly enriched our knowledge of Swedish emigration through this report. The maps, graphs, tables, and bibliography add substantially to the usefulness of this fine volume. The English translation, principally by Kermit Westerberg, is clear and readable.

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GUSTAF JONASSON. *Per Edvin Sköld, 1946–1951*. With the assistance of LARS SKÖLD. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 77.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 203.

Per Edvin Sköld, a leading Social Democrat in Sweden, held important offices in the government for twenty-three years. During World War II he served as minister of defense. He was more determined to follow a policy of strict neutrality than the prime minister himself, Per Albin Hansson, who yielded to German pressure for the privilege of transit across Swedish soil to German-occupied Norway. From 1946 to 1951 Sköld headed, successively, the departments of agriculture, economic coordination, and finance. He may be remembered as having been, at his peak, the strongest political force in the government. He retired from the Riksdag in 1964 and died in 1972.

This specialized work devotes chapters to, among other topics, the successors of Per Albin Hansson, internal cabinet crises, Nordic defense, and the coalition with the Agrarian party (known as Centrist after 1957). A long final chapter contains Sköld's personal notes from 1946 to 1951. Sköld makes little reference to the United States; nor does he allude in any significant way to cold war developments such as the Truman Doctrine, the Berlin blockade, or the Czech revolt.

Nordic defense occupies a prominent place in Gustaf Jonasson's treatment. Prior to the organization of NATO (April 4, 1949), which Sweden did not join, several meetings of Danish, Norwegian, and Swedish ministers took place. Sköld agreed, in the main, with Sweden's new prime minister, Tage

Erlander, and with foreign minister Östen Undén in rejecting participation in great-power politics and in favoring Nordic solidarity instead. Nordic cooperation was permanently achieved in 1952 with the creation of the Nordic Council, although Sweden continued to remain outside the Atlantic alliance.

Both Gustaf Jonasson's commentary and Sköld's diary emphasize conflicting views in Oslo. Norway's Social Democratic prime minister, Einar Gerhardsen, appreciated Sweden's position on joint Scandinavian defense. His foreign minister, Halvard Lange, whose judgment Sköld distrusted, leaned more toward the West. (Neither Gerhardsen's memoirs nor Lange's public utterances are cited in this work.) In any event, the Norwegian government followed Lange's line and joined NATO, refusing to go along with Gerhardsen on a common Nordic defense policy.

This useful study concludes with a listing of mainly Swedish archival and other sources, selected readings, an index of names, and a compilation of the seventy-six previous publications in this invaluable Uppsala University series. American scholars would benefit by its translation into English.

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D. G. KIRBY, editor and translator. *Finland and Russia, 1808–1920: From Autonomy to Independence. A Selection of Documents*. New York: Barnes and Noble, 1975. Pp. xiii, 265, \$27.50.

The relationship of Finland and Russia during the nineteenth and twentieth centuries is an aspect of European history which has remained obscure and baffling to many historians. Thus a young British scholar, D. G. Kirby, has done a genuine favor to students of European history by compiling and translating this collection of documents pertaining to Finno-Russian relations. Actually the selection of documents included is even somewhat broader than the title seems to promise. For example, Kirby has included a whole section to illuminate the complex situation between the Finnish-speakers and the Swedish-speakers of Finland. On the surface its inclusion in a collection devoted to the Finno-Russian relations may appear odd to some readers. It is, however, fully justified and quite proper. The language issue was a central one in nineteenth- and twentieth-century Finnish history, and as such it was also directly intertwined with the whole Finno-Russian relationship.

The emphasis of Kirby's collection lies quite justifiably in the last three decades of the period treated, namely from the 1890s to the 1920s. The

documents are classified into the three principal divisions, which he characterizes as the periods of autonomy; autonomy threatened; and revolution, civil war, and independence. Within this chosen chronological framework the selections seem well balanced, and the translations are clear.

Kirby has also written a brief and concise introduction in which he attempts the formidable task of covering on some six pages those aspects of Finnish history that his documents illuminate. While discussing the complexities of nationalism in Finland the author might also have discussed, in addition to the ethnolinguistic nationalisms of the Finnish-speakers and the Swedish-speakers, the "Runebergian" or the "Topelian" kind of general patriotism or nationalism that embraced and transcended the narrower ethnolinguistic nationalisms. Besides the general introduction there are also briefer one- or two-page introductions to the subdivisions in which one might question a point or two. As a whole the introductions are quite well thought out and done; Kirby strives and largely succeeds in the difficult task of being fair and balanced in treating highly controversial topics within strict space limitations. All in all, this compilation is a welcome addition to the growing English-language literature in Finnish history and gives one reason to look forward to Kirby's future contributions in the field, for example a continuation volume.

PEKKA KALEVI HAMALAINEN  
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PETER WENDE. *Radikalismus im Vormärz: Untersuchungen zur politischen Theorie der frühen deutschen Demokratie*. (Frankfurter Historische Abhandlungen, number 11.) Wiesbaden: Franz Steiner Verlag, 1975. Pp. 228. DM 36.

Of the various political parties which began to emerge in Germany in the decades before 1848, the radicals are the least known. This is due partly to their numerical weakness, and partly to their failure to establish a vigorous tradition in the public life of Central Europe. Peter Wendt has now helped fill this gap in our knowledge by writing a book, a *Habilitationsschrift*, to be exact, dealing with their civic beliefs. He has examined six prominent figures of the Left in order to discover the attitudes characterizing the movement as a whole: Arnold Ruge, Julius Fröbel, Johann Georg August Wirth, Gustav von Struve, Karl Hagen, and Karl Nauwerck. From a study of their writings he has extrapolated a systematic statement of radical views on the relationship of the individual to state and society, on social criticism and social reform, and on the interconnection between history and



politics. He has done his work skillfully, so that the result is a persuasive analysis of a small sample of the radicals, a sample which presumably represents the Left in its entirety.

This is a study in the history of ideas in the abstract. Within the framework of the categories which the author has applied, he is perceptive and informative. The trouble is that he deals with issues about which we know the most, ignoring those we understand the least. No one can quarrel with what the book has to say about Fröbel's organized community of purpose or Ruge's concept of the absolute state. But would we not be better served by learning more about the concrete program of the radicals, about the solution they proposed for the political problems facing their nation, and the stand they took on the economic issues of the day? How did they feel about factories, guilds, entailed estates, and manorial obligations? How did they vote in the state legislatures in which they were members? What did they have to say in their newspaper articles about the factional controversies which are the lifeblood of parties? Perhaps it is unfair to complain that an author has not written a book different from the one which he has in fact written. In terms of what he tries to achieve, Wende makes a contribution to our understanding of the background of 1848. But it is a pity that we must still wait for some future work, based on an examination of the parliamentary debates of the secondary states and a scrutiny of the periodicals of the Restoration, to tell us what the radicals thought about the down-to-earth economic and social problems of their time.

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GERHARD SCHULZ. *Aufstieg des Nationalsozialismus: Krise und Revolution in Deutschland*. Berlin: Propyläen. 1975. Pp. 921. DM 68.

This is an important and extremely welcome book of synthesis. Specialists will perhaps find few surprises, but all scholars are indebted to Gerhard Schulz for placing the rise of Nazism into a much broader analytical context than was the case in previous efforts. To be sure, there are new details as well (e.g. more information on the size of industrial contributions to the SA), but the book's major value lies in tracing the Nazis' rise to power against the dual backdrops of the post-1890 European malaise and its specific characteristics in Germany. What later came to be known as Fascism and National Socialism had its origins in the breakdown of the traditional interdependence of social classes in the late nineteenth century. These

developments were in turn vastly accelerated by the First World War and the Russian Revolution, leading in turn to the emergence of a new form of revolutionary conservatism, characterized by a predilection for activism and violence and, in reaction to the events in Russia, by a distinctly social (but not socialist) component.

The peculiarities of German political and intellectual history gave the new conservatism a number of specifically German characteristics, among them its romantic components, the idealization of Prussia, and the concern with the problems of the *Mittelstand*. Above all, however, the German new conservatism was fiercely and almost irrationally antiliberal and antiparliamentary, substituting plebiscitism for liberalism and authoritarianism for parliamentarism. The Nazis began as a part of the new conservatism; they outdistanced their rivals after the onset of the economic depression by simultaneously personalizing politics (through the Hitler cult) and creating a party program that turned the *Mittelstand* into a mass political force. (Schulz largely credits Goebbels with successfully engineering both developments.)

As noted above, most of this is not new or original. The successful integration of the large number of developmental strands makes this book, however, the new standard account of the politico-intellectual climate that led to Hitler's rise to power. In addition, the book provides footnotes and a bibliographic apparatus that should provide starting points for many follow-up dissertations and research projects.

If there are any conclusions that will arouse major controversies, they will probably be Schultz' de-emphasis of the role of anti-Semitism in the Nazis' rise, and his treatment of political Catholicism and the Center party in post-World War I Germany. Schulz sees the right wing of the Center party (and the party's national leadership after 1928) as very much in the mainstream of the new conservatism. Brüning in particular worked systematically for the introduction of an authoritarian, military-dominated dictatorship as a prelude to a restoration of the monarchy. In these efforts Brüning deliberately misled the SPD as to his ultimate intentions.

I have no substantive criticism of the book, although there are times when the argument tends to get obscured by masses of detail and heavy-handed academic style.

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RADOMÍR LUŽA. *Austro-German Relations in the Anschluss Era*. Princeton: Princeton University Press. 1975. Pp. xvi, 438. \$20.00.



The Anschluss question has recently attracted considerable attention among journalists (Dieter Wagner and Gerhard Tomkowitz) and historians (A. D. Low and Stanley Suval) as well as political scientists (Peter J. Katzenstein). But Radomír Luža has no reason to apologize for his use of "conventional" methods. His study of the Anschluss era is an exemplary piece of historical scholarship that sheds much light on the problem of Austro-German relations as well as on the nature and policies of the Third Reich.

In view of Hitler's ponderous forecast of the "Thousand Year Reich," his "empire" got off to a lamentable start. The annexation of Austria, entirely in line with his ethnic premises, should have been an exhibition piece of Nazi imperial policy. In fact it was one of Hitler's vacillations, a tug of war between the central administration in Berlin (Frick) and the *Reichskommissar* (Bürckel), and in turn between the latter and the *Reichsstatthalter* (Seyss-Inquart), to a point where "the chaos and constant infighting was perceived and accepted as an almost inevitable part of the system."

Once all this is said, due credit must be given to the inevitable "cunning of reason." In 1938 Hitler may have destroyed his native Austria; but his decision to maintain most of the *Länder* in the form of *Reichsgaue* allowed the latter to become channels of regionalism and indeed of Austrian traditionalism. Furthermore, the German economic annexation, while for the moment virtually dispossessing the Austrians and subjecting the area to painful market vacillations, had long-range effects which must be measured in more than economic terms. Just as the Nazi "push into modernity," according to Ralf Dahrendorf, removed the basis for future German authoritarian governments along traditional lines, so the rule of National Socialism in Austria laid the basis of a modern industrial society and cleared the way for economic and social progress. Altogether, Austria has found its national identity as well as its full access to modernity through occupation and defeat.

Luža's exhaustive research and discriminating approach to his problem are dictated by the uniqueness of the Austro-German problem in modern European history. Peter J. Katzenstein, in his recent attempt (*Disjoined Partners: Austria and Germany since 1815* [1976]) to develop the Austro-German relationship in a broader context, is ingenious but not convincing. His thesis concerning the triumph of fragmentation over integration is based on the loosening ties between Sweden and Norway, England and Ireland, the U.S. and Canada, and so forth, a model which does not fit the Austro-German case in which in roundabout ways the dynamic force of nationalism of the Herderian variety was overcome by a Western type of state consciousness. In his unassuming way Luža has

convincingly reaffirmed the case for the historical method.

KLEMENS VON KLEMPERER  
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ROBERT CECIL. *Hitler's Decision to Invade Russia, 1941*. (The Politics and Strategy of the Second World War.) London: Davis-Poynter. 1975. Pp. 192. £4.50.

This short, well-written account is part of a series on the politics and strategy of World War II. In 1972, Robert Cecil wrote *The Myth of the Master Race: Alfred Rosenberg and Nazi Ideology*. He draws heavily from that study in this account of the motivation and planning behind Hitler's decision to strike at Russia.

Cecil correctly asserts that Hitler's decision to invade Russia was not only one of the most fateful of this century but also one that was most in keeping with his ideological preconceptions. The author seeks to show the evolution in Hitler's mind of the ideas and plans that made this step inevitable. The stress throughout is on the ideological motive. While this idea, in turn, rested on the peculiar racial and Social Darwinian ideas that had existed in Germany for many generations, it was, for Cecil, Hitler's peculiar function to give them a demonic life of their own. Hitler's ideological preconceptions and not economic or strategic considerations thus governed the Nazi decision to strike.

The book is clear, concise, and easy to read. The sources are used well. It is best appreciated with a clear understanding of Hitler and Rosenberg's writings. Indeed, one of the few criticisms one might make is that Cecil may overemphasize Hitler's paternity of the ideological crusade. I wonder how Hitler's thinking on Russia was influenced by having people like Rosenberg and Himmler near him. At a time when many books on the last world war are appearing, I would think this short volume would recommend itself well to anyone who wanted a clear statement of the emergence of the ideological factor in Nazi military strategy.

BROOKS VAN EVEREN  
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ERNA LESKY, editor. *A System of Complete Medical Police: Selections from Johann Peter Frank*. Translated by E. VILIM. Baltimore: Johns Hopkins University Press. 1976. Pp. xxiii, 469. \$18.50.

Each year the degree to which eighteenth-century medical historiography lags becomes more apparent as outlines emerge for a new picture of that "age of schools and systems, devoid of discovery." Recent studies have amply demonstrated that Enlightenment medicine was the seedbed of new con-

cerns about the relations between medical and broader cultural institutions. Medical men began concertedly to address childhood, mental illness, and the mercantilist equation of health and the wealth of nations. Political and intellectual structures were devised to accommodate their solutions.

The catalyst which helped fuse these elements of concern to medicine and make them inseparable in the minds of autocratic rulers and medical men alike was the concept of "medical police." Its apotheosis lay in the monumental collection of broadsides published between 1779 and 1819 by the Viennese physician, public health man, and prototypic medical bureaucrat, Johann Peter Frank.

Erna Lesky has now provided an adequately extensive abridgement, the first such in English, of Frank's magnum opus, accompanied by a useful biographical introduction and scholarly apparatus. Distilled into the new edition is the full range of a remarkably expansive vision of public medicine. For Frank, *medizinischen Polizey* ramified not only into those areas anticipated by more modern sensibilities, such as sanitation or maternal-child welfare, but also traffic safety, housing construction, public amusements, early marriage, noise pollution, regulation of drug and poison manufacture, suicide, gambling, sorcery and witchcraft, child labor, or medical education.

At the end, the reader who diligently makes the journey through Frank's sometimes turgid prose will be particularly rewarded. The author's treatment in the sixth and last volume of institutions of medical learning, of the interface between health education and health care, is vigorous and provocative. In all, this is a unique documentary source for late eighteenth-century medical history.

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ARIEL TOAFF. *Gli Ebrei a Perugia*. (Deputazione di Storia Patria per l'Umbria. Fonti per la storia dell'Umbria, number 10.) Perugia: the Deputazione. 1975. Pp. 342.

This work adds to the growing number of recent studies devoted to the history of Italian Jewish communities in, for example, Florence, Mantua, Padua, Venice, and Rome. It surveys chronologically the history of the Jews of Perugia from their earliest settlement during the mid-thirteenth century until the present, with emphasis on the period of their greatest concentration in the fifteenth century. Ariel Toaff presents a competent and readable account of a major Jewish community of the Papal States. He has considered all available archival sources (he adds significantly to the previous findings of A. Fabretti) as well as manuscript sources and provides a great service to scholars by

publishing a large sampling of the sources discussed in his narrative.

Toaff correctly identifies two major problems of research in Italian Jewish history. Earlier surveys have attempted to draw conclusions prematurely from a too limited number of sources without paying sufficient attention to the wealth of available data concerning specific communities and individuals. Moreover, earlier historians have not successfully integrated the Latin and Italian archival sources at their disposal with the more inaccessible Hebrew manuscript sources. In this volume, the author attempts to correct both deficiencies by focusing on one specific community and by carefully weighing both kinds of evidence.

Notwithstanding this achievement, the study has its limitations. Modeling his work after Cassuto's masterful treatment of Florentine Jewry, Toaff focuses on the cultural achievement of Perugian Jewry. Because of the geographical limitations of his study, however, his discussion of major figures and ideas is severely constricted. Given the inter-regional nature of Italian Jewry, the mobility of its persons and ideas, Toaff's treatment of cultural history (unlike Cassuto's, which focused on major intellectuals essentially involved with Florentine culture) represents no more than a catalog of names and dates and a rehashing of previous research. Furthermore, can the argument that we are not ready for a synthesis obviate the responsibility of the researcher to make any generalizations? A concluding tentative comparison with other communities, utilizing already published studies, would have strengthened the entire work. Toaff's data on Jewish banking, communal organization, and intellectual life could be better evaluated if compared with similar data of other communities. By not placing his communal history in a broader context, the author fails to demonstrate why his subject is significant.

Finally, Toaff gives us only a superficial treatment of the city itself and its non-Jewish population. The unique role of Perugian Jewry could have been more clearly illuminated by considerations of the political and religious life of the Papal States in relation to the rest of Italy, the particular interest groups of Perugia, and the cultural achievements of the community as a whole.

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L. R. LIND. *Studies in Pre-Vesalian Anatomy: Biography, Translations, Documents*. Philadelphia: American Philosophical Society. 1975. Pp. 344. \$18.00.

This volume makes available a great deal of information and documents related to eight Italian

anatomists who published their works prior to the appearance of Vesalius' *De Humani Corporis Fabrica* in 1543. L. R. Lind has extensively researched Italian archives and libraries and carefully translated a series of anatomical texts from the Renaissance Latin.

The reader is greeted with a useful introduction that briefly depicts the cultural background of the early sixteenth century. Like others before him, Lind wrestles with the question of the prominence accorded to Vesalius contrasted with the obscurity of his immediate predecessors. What made Vesalius such a great innovator? A biographical analysis of his near contemporaries discloses that they were extremely busy teachers and physicians with many interests and multiple duties. Lind detects a lack of specialization that sharply differs with Vesalius' single-minded purpose and concentration in anatomical studies. Another factor clearly distinguishing Vesalius' predecessors was their lack of interest in illustrations and the poor printing quality of their books. Above all, Lind advances the idea that these men did not display the necessary courage to challenge the authority of the ancients, especially Galenic anatomy.

The book is divided into eight parts, each of which deals with the life and works of a pre-Vesalian anatomist. The biographical and bibliographical essays are followed by brief annotated translations of their works.

Lind deals in succession with Alessandro Achilini (1463-1512) of Bologna, whose *Anatomical Notes* (1520) he presents in English, followed by Alessandro Benedetti (1450-1512) of Verona and his *Anatomice* (1497). Next come Gabriele Zerbi (1485-1569), also of Verona, and Berengario da Carpi (1470-1530) with his *Commentary on Mundinus* (1521). The other authors included are Niccolò Massa (1485-1569) and his *Introductory Book of Anatomy* (1559), Andrés de Laguna (1499-1560) and *Anatomical Procedure* (1535), Johannes Dryander (1500-1560) with *The Anatomy of the Human Head* (1535), and Giovanni B. Canano (1515-1579) with *An Illustrated Dissection of the Muscles of the Human Body* (1541).

The author includes an appendix that contains letters and testaments belonging to some of these men. An extensive bibliography of primary and secondary sources is also appended. The only glaring omission is Lind's failure to analyze the renewed interest in anatomy in the Italian city states, especially after 1500. The possible role played by surgery and legal issues is not debated nor does the author discuss the changes in social values which allowed the resumption of human dissections. Was this related to the impact of massive epidemic disease after the fourteenth-century "Black Death"?

Lind has brought together valuable information

regarding some of Vesalius' most prominent predecessors from a great variety of archival sources. His labors directed toward a complete history of pre-Vesalian anatomy will be widely appreciated.

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VALERIO MARCHETTI. *Gruppi ereticali senesi del cinquecento*. (Biblioteca di storia, number 19.) Florence: La Nuova Italia. 1975. Pp. xii, 294. L. 3,500.

ANTONIO ROTONDÒ. *Studi e ricerche di storia eretica italiana del cinquecento*. Vol. 1. (Pubblicazioni dell'Istituto di Scienze Politiche dell'Università di Torino, number 31.) Turin: Edizioni Giappichelli. 1974. Pp. 584. L. 9,500.

These are two substantial and highly individualized contributions to one of the most exciting areas of Reformation studies: the Radical (inclusive of Italian) Reformation.

Marchetti, who has obvious affinities with the Frankfurt school, combines prodigious archival research, a phenomenological method, a sociological sense, and a flair for drama—in which none is sacrificed to any other. His indicated objective is "to reconstruct the physiognomy" of Siennese dissent between 1543 and 1560. He does this brilliantly, revealing tensions between various religious groups and the strata of society, without doctrinaire heavy-handedness. He introduces us to fascinating personalities like Basilio Guerrieri, the ideologist of Siennese Calvinism. Guerrieri advanced a policy of what we might call Fabianism, the gradual penetration of existing institutions, that is, the control of the confraternities by the acquisition of their key offices. A champion of orthodoxy, Guerrieri avoided extreme elements like the anti-Trinitarians on the one hand, while on the other he openly informed the Inquisition about a Dominican who taught the universality of God's salvific will. He is a perfect model for Robert Kingdon's analogy of Geneva as the Third International of the sixteenth century.

Interestingly, the Calvinist doctrine of election reinforced the clandestinity of Siennese Protestants: their concern was not the empirical church anyway. And during this period, Protestantism seems to precede humanism. Reformation students will despair to learn there are more than a dozen Sozzini. All the same, final repression in 1560 presented the alternatives of Nicodemism—closet Protestantism—or exile in *terra riformata* of the North.

Rotondò, a distinguished student of Eugenio Garin and the late Delio Cantimori (and like them, a Marxist) and an editor of the *Corpus Refor-*

*matorum Italicorum*, takes us from Siena to the larger European scene. His admirably researched essays, some of which have been previously published, deal with such problems as Calvin and the anti-Trinitarian Italians, an argument with Jerome Friedman on Socinianism, interesting excursions into lesser-known figures like Francesco Pucci, Pietro Perna, and Agostini Doni, and even one on "the French prophet and visionary" Guillaume Postel.

The hero of the book is clearly George H. Williams, and Rotondò begins with an appreciative and extensive profile on Williams and the place of his monumental *The Radical Reformation* in scholarship. While commending Williams for unifying the field, Rotondò also endorses the famous typology of Ernst Troeltsch (church, sect, mystic). Several essays are dedicated to controversy on the anti-Trinitarians, and the author issues a sharp reply to his critic Jerome Friedman. Essentially, Rotondò rejects any linear development between Servetus and Socinianism in favor of one that runs from Lelio Sozzini to his nephew Fausto. Rotondò can quote Fausto in support, and the apparently impartial opinion of E. D. Willis in the *Archiv für Reformationsgeschichte* (1971, pp. 279-82) would offer more comfort to Rotondò than to Friedman. Rotondò is really no monocausalist; in his preface he promises to relate the development of heresy to the entire religious and cultural life of the *cinquecento*: to Hermeticism and the Cabala, the "dream" of social revolution, debates over witchcraft, astrology, and magic. George Williams, in the *Festschrift* to Harold Grimm has, incidentally, already uncovered new evidence of Judaizing impulses among Italian religious radicals. I take it that the longest essay in the present volume, that on Perna, an Italian expatriate at Basel, who published Paracelsus and a controversial anonymous work that rings of Agrippa's *De occulta philosophia*, is not considered evidential. Rotondò, who offers suggestions for further research, promises to explore some of these matters in a subsequent volume. This gives us something to look forward to.

Both works should be indispensable for Italian and Reformation historians. Interestingly, the radical social orientations of the authors do not lead to a revival of any facile economic interpretations but to a rigor and sweep that some American scholarship, still captive to a seminary sacrosanctity, lacks.

DONALD NUGENT  
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FRANCA ASSANTE. *Città e campagne nella Puglia del secolo XIX: L'Evoluzione demografica*. In *Cahiers Internationaux d'Histoire Économique et Sociale*. Volume 4. (Istituto Italiano per la Storia dei Movimenti Sociali e delle Strutture Sociali.) Geneva: Librairie Droz. 1975. Pp. xii, 445. 70 FR.

Franca Assante's study of the relationships between the "city" and the "countryside" in nineteenth-century Apulia has two main objectives: to specify the problems faced by the Apulian population and to identify the factors which conditioned the region's economic development. Assante maintains that, despite the high level of urbanization, Apulia did not experience a genuine distinction between its urban and rural sectors. Rather, the urban population was composed primarily of rural workers who lived as "suburban peasants." Assante also found that Apulia's economy relied almost wholly on agriculture, particularly on wine, olive oil, and grain. While these products were profitable in the short term, they necessitated long-term investments in the land. They also proved to be extremely vulnerable to outside forces. For example, during the 1880s, when trade relations with France were disrupted, Apulian wine producers were deprived of their main market, which resulted in the collapse of the Apulian wine industry and adjacent industries, a drop in the value of real estate, high unemployment, and the reduction of salaries by up to sixty percent. Paradoxically, it was crises such as this that forced the rejection of traditional attitudes and the search for new directions in development.

Assante has successfully avoided monocausal explanations by emphasizing the effects of traditional methods of agriculture, economic and demographic patterns, geographic conditions, national and international developments, the lack of adequate credit institutions and investment capital, and the absence of a strong entrepreneurial spirit among the Apulian middle and upper classes. Nevertheless, she does indict decision-makers for failing to link the national railway system to the internal provinces of Apulia, for failing to link the centers of production to the centers of consumption within the region itself, and for failing to adjust taxes and duties to the local ability to pay.

This book engages the reader in a carefully researched and balanced discussion of the human and structural dimensions of regional disparity and economic development. It cannot but be of interest to economic, demographic, and social historians.

ELMIRO ARGENTO  
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ANGELO GAMBASIN. *Religione e società dalle riforme napoleoniche all'età liberale: Clero, sinodi e laicato cattolico in Italia*. (Biblioteca di Cultura.) Padua: Liviana Editrice. 1974. Pp. xvi, 259.

Angelo Gambasin is one of a number of Italian scholars who in the last decade have made study of the Catholic Church the basis for a new look at Italian society in the nineteenth century. This vol-

ume, which complements his detailed study of parishes in the Veneto published last year, ranges from the effects of Napoleonic reforms on the clergy to the formation of the *Società Cattolica per gli Studi Scientifici* in 1899. The essays it contains are typical of what is becoming a historical school in the evidence used, the approach to that evidence, and the broader questions opened up.

Most of the five chapters here were written initially for scholarly conferences, and they are marked by the rich prose, dense style, and learned asides of that genre. Each rests primarily on institutional sources, mainly records of Church synods or councils of bishops. The approach to those sources is in the tradition of Italian historical idealism, with long quotations used to establish a kind of textual analysis of the flow of ideas. The initial perspective then combines institutional and theological emphases in patterns well established in Church history. The questions Gambasin implies, however, have to do with social change, for he is curious about folk religion and popular piety, hygiene and education, demography and factory labor. If he offers no theory as to what the intrinsic connections among such matters may be, he does provide a sensitive study of how social change is filtered through the institutional needs and problems of the Church and the *mentalités* of nineteenth-century bishops. The effect is both to open up new bodies of evidence and, more important, new questions, the pursuit of which will require dozens of other studies.

The picture that emerges is of the Church institutional accepting the bureaucratization and specific official functions of the Napoleonic period, gradually restive with the secular role assigned it by the Austrians, but limited by circumstance in the range of its response. He shows bishops impressively aware of the changes around them from the 1840s on, increasingly fearful of those changes, and tending defensively to rely upon institutional strength, intolerance, and systematic restatement of dogma. Yet even these responses include an impressive social awareness that leads, bit by bit, to the insistence that Catholicism must contain within it answers to the social issues of the day. Resting at first on myths of medieval corporatism, this insistence led to hesitant but wide-ranging interest in social action, and, among conservatives and moderates alike, to devastating attacks upon liberal society. If the newer questions about the interconnection of institutions, religious belief, and social change raised by Gambasin are ever answered, we will know a great deal more than ever before both about the process of change and about the essential networks of Italian society.

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GIUSEPPE MONSAGRATI, *Federalismo e unità nell'azione di Enrico Cernuschi (1848-1851)*. ("Domus Mazziniana," Collana scientifica, number 15.) Pisa: Nistri-Lischi, 1976. Pp. 289. L. 4,000.

Enrico Cernuschi has always been considered a minor figure in the history of the Risorgimento and has usually been relegated to the footnotes of major works. A young man distinguished primarily by his foppish dress and sarcastic wit, he served with Cattaneo on the War Committee which directed the Five Days Insurrection in Milan in 1848. After participating in the unsuccessful defense of Milan, Cernuschi went to Rome where he served with the Roman Republic, helping to organize the defenses of Rome against the French, who captured it in 1849. Arrested by the French, he spent a year in prison, finally gaining his freedom after being absolved in two clamorous trials. He then emigrated to Paris where he was briefly involved in the convoluted machinations of exile politics, siding with Giuseppe Ferrari and joining Giuseppe Montanelli's short-lived "Latin Committee" that was to liberate France, Spain, and Italy simultaneously. Cernuschi renounced active politics after Louis Napoleon's coup of December 1851, however, and devoted his energies to a career in finance and banking; he became a French citizen and never played any part in Italian politics thereafter.

Giuseppe Monsagrati's study proposes to "document the gradual deterioration of relations between Mazzini and Cernuschi, the representative of democratic federalism" (p. 14) and thereby come to a deeper understanding of the dissidence among Italian democrats that followed the failures of 1848. Cernuschi is a strange choice for such an analysis, since he is such a minor figure compared to Ferrari or Cattaneo. Monsagrati's work does nothing to rehabilitate him, and he even says Cernuschi "lacked political sense and flexibility."

But Monsagrati is not really trying to enlighten us; instead, by choosing the weakest representative of democratic federalism he shows us how clearly superior the approach of Mazzini was, and how Cernuschi's dissidence in the end only served to divide democrats and thus indirectly to support the Cavourian moderates. While this book has nothing to offer contemporary historians in the way of evidence or insights, it is a remarkable monument to the ability of Italians to sustain a political argument now more than 125 years old.

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ANTONIO CONFALONIERI, *Banca e industria in Italia, 1894-1906*. Volume 2, *Il sistema bancario tra due crisi*. (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana, 1975. Pp. 515.



This second volume of Antonio Confalonieri's study concentrates upon the Italian banking system from the financial crisis of the early 1890s to the eve of the Depression of 1907. It examines the bank law of 1893 and the emergence of the Banca d'Italia, tracing its development into a central bank that regulated the money supply and acted as guardian of the country's economic interests. It also delves into the traditional sector of the banking system, the savings banks (*casse di risparmio*) and the cooperative banks (*banche popolari*). The broadest treatment, however, is given to the "mixed banks," which did a business similar to those of the German banks, offering both short-term commercial loans as well as long-term loans for the development of industry.

Confalonieri stresses the role and activities of three industrial credit banks: the Società Bancaria Italiana, the Banco di Roma, and the Credito Italiano, making frequent comparisons to the fourth and most important credit bank, the Banca Commerciale Italiana, to which the author has devoted his third volume. Noting the primacy of the Banca Commerciale and the Credito Italiano, Confalonieri suggests that this led the Banco di Roma to seek industrial opportunities abroad and the Società Bancaria to invest in less than solid enterprises.

For the two most important banks operating on the German model—both of which were initially financed largely by German capital—the author has relied upon private as well as public papers and therefore provides a well-rounded treatment. His description of the other institutions is drawn almost exclusively from public documents and is therefore more one-dimensional. He has put the material available to good use, however, and this volume, like its predecessor, *Le premesse*, is well researched and supported by a wealth of statistics and notes.

In light of the general title of the work, as well as of the fact that the interrelationship between banking and industry has not been as well studied in the Italian case as in the French or German, I was disappointed that the volume stressed the development of the banking sector and only secondarily examined the effects of banking upon the rate, direction, and concentration of Italian industrial expansion. Still, there is much here on this second theme. Unfortunately, the emphasis on the institutions has also meant that the political ambience is often neglected, so that one would not surmise from this book that Giovanni Giolitti dominated the political life of the country for many of these years or that he played an important part in its financial and industrial development.

FRANK J. COPPA  
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MARIO TOSCANO. *Alto Adige—South Tyrol: Italy's Frontier with the German World*. Edited by GEORGE A. CARBONE. Baltimore: Johns Hopkins University Press. 1976. Pp. xii, 283. \$15.00.

This translation of *Storia diplomatica della questione dell'Alto Adige* (originally published, 1968) is a detailed chronicle of the acrimonious dispute between Italy and Austria over the status of that German-speaking region south of the Brenner Pass from 1914 to 1969. The volume, which is very pro-Italian and often quite polemical, is based on lectures given in 1960–61 and 1966–67 by the late Mario Toscano, a specialist in the history of treaties and international politics, in the School of Political Science at the University of Rome. The book confines itself to diplomatic history. Toscano's long-standing connections with the Italian Foreign Ministry gave him access to relevant archival material in Italy. Moreover, the widow of former Premier Alcide De Gasperi permitted him to consult the latter's personal files relating to the background and aftermath of the important De Gasperi-Gruber Agreement of 1946. For the final chapters, Toscano drew also upon his own experience as delegate to the U.N. General Assembly meetings after 1956. Despite the thoroughness of his coverage, the author curiously omits even passing mention of Mussolini's mobilization of troops at the Brenner Pass in 1934 when he feared a Nazi putsch was imminent in Austria.

Toscano decided to publish his version of the international dispute after a newspaper correspondent, Karl Heinz Ritschel, brought out his *Diplomatie um Südtirol: Politische Hintergründe eines europäischen Versagens* (1966). That volume contained many documents belonging to the late Italian Senator Ettore Tolomei (who had pushed the Fascist program of Italianization of the Alto Adige) as well as hitherto confidential minutes of conversations between the Italian and Austrian foreign ministers. Thus the chronological scope of Toscano's book was largely determined by Ritschel's example, the terminal date being March 1965—the time of Vienna's failure to endorse the first Italian Comprehensive Outline of Approach toward a Settlement. Later, Toscano worked on a supplemental text. At the time of the author's death, September 17, 1968, the rough draft of this supplement covered the negotiations down to January 21, 1967. This material, revised with the help of Giuseppe Vedovato and the author's daughter, has been included in this English translation as chapter 9. The positive accomplishments that had been registered in Austro-Italian negotiations by December 1969 suggested to the coordinator of the translation the desirability of including an additional outline as chapter 10.

The 1969 agreements promised to broaden within the framework of the Trentino–Alto Adige

region of Italy the legislative and administrative powers of the German-speaking province of Bolzano (Bozen) and to include the possibility of recourse to the Hague Court in case of disputes over interpretations of the De Gasperi-Gruber and other agreements between Austria and Italy. These were approved by parliaments of both countries, though narrowly in the case of Austria. Since then there has been considerable lessening of tensions. Toscano's ex parte book can be regarded as the authoritative Italian version of the prolonged dispute.

CHARLES F. DELZELL  
Vanderbilt University

SIDNEY SONNINO. *Carteggio 1914-1922*. Volume 1, 1914-1916; volume 2, 1916-1922. Edited by PIETRO PASTORELLI. Bari: Editori Laterza. Lawrence: University Press of Kansas. 1974, 1975. Pp. xvi, 770; xv, 757. L. 11,500; L. 14,000.

These two volumes, best read in connection with volumes 2 and 3 of Sonnino's *Diario* covering the same period, complete the remarkable piece of detective work done by Benjamin F. Brown, who located the Sonnino papers in a back room of his Tuscan villa of Montespertoli. They consist mostly of documents covering the period of the First World War extended to Sonnino's death in November 1922.

The Sonnino archive proper is supplemented by other sources, for example, the Salandra papers. Relations between the two men, both of conservative inclination, were close, far more so than those between Sonnino and Salandra's two successors. Many of the documents are available in such earlier publications as those of Crespi and Aldrovandi Marescotti, and the more recently issued relevant volumes of the *Documenti Diplomatici Italiani*. Consequently there is not a great deal that is new in the work, the chief merit of which lies in the central focus of Sonnino himself.

The division into two volumes is logical, the first reaching the end of the two Salandra ministries in June 1916, the second covering the tenures of Boselli and Orlando, plus the above-mentioned addition to 1922. Sonnino's reputation of forthright rigidity is borne out in this record. If limited, his vision of the Italian interest was clear and he defended it with tenacity. Italy being a Mediterranean power, his ambition was essentially confined to the eastern section of that sea, the proximate Balkans and the Ottoman Empire. Hence the attention given to such problems as Albania, the hopes of the South Slavs, and the efforts to coordinate Italian policy with Romanian.

This narrowness of concern may explain certain gaps. Surprisingly little appears on the Russian

collapse and its implications, and it is strange to find no indication of any discussions with President Wilson on the occasion of his visit to Italy at the end of 1918. Sonnino was fully aware of the implications of the Wilsonian program though he seems to have hoped for a time to reconcile it with the Italian expectations written in the 1915 Treaty of London.

The last section, though brief, is not the least interesting part of the work. It is largely made up of exchanges between Sonnino and Bergamini, the editor of the *Giornale d'Italia*, which was commonly regarded as Sonnino's mouthpiece during his tenure of office. The appraisal of the condition of Italy has a very contemporary ring. That was the reason Sonnino could accept the Treaty of Rapallo, Sforza's solution of the Italo-Yugoslav impasse. It was likewise the reason he accepted the advent of Fascism as a possible solution despite his reservations about the competence of its leader. One can only regret that editorial necessities have prevented the inclusion of an additional volume, which would have made the record fuller. As it is, we are in Brown's debt.

RENÉ ALBRECHT-CARRIÉ  
Columbia University

A. JAMES GREGOR. *Interpretations of Fascism*. Morristown, N.J.: General Learning Press. 1974. Pp. v, 281.

This text for college classes should be entitled *Interpretations of Italian Fascism*. It evaluates the major social science explanations of Mussolini's movement, although A. James Gregor attempts to broaden its significance by appointing Italian Fascism a "paradigm" of generic Fascism: "If an effort at explanation fails to plausibly account for Italian Fascism its credibility will be considered compromised." Some readers will protest this method, but no matter: these days all historiographical surveys of Fascism seem to have the same result. Writes Gregor: "We simply do not have, as yet, a compelling interpretation of Fascism".

Nevertheless, he believes that the classic interpretations of Fascism as the consequence of class struggle, moral crisis, psychological disabilities, or the rise of "amorphous masses" have provided some "negative insights." Stated plainly, he means that most of them are "hopelessly inadequate" and that by now we should know better. Reduced to the packaged summaries offered here, these popular theories do indeed appear vacant and flimsy. After these quick kills, however, students will be impatient with the labored descriptions of the two remaining interpretations: Fascism as a stage of modernization and a form of totalitarianism. Here the book is less a survey for beginners than a debate with scholars, a rearguard de-

fense of ideas that shaped Gregor's earlier works on Italian Fascism and totalitarian thought. Indeed, one value of the present volume is to reveal unconsciously the apprehension resulting from the present disintegration of modernization theory. For Gregor, it is still the "maximally plausible" interpretation; but at best, he acknowledges, it provides only "discursive generalizations": vague, ambiguous, and nontestable.

Gregor offers his book in the hope that his readers may learn something about both social science and Fascism. Regrettably, we learn that social science teaches us little about Fascism, that what it teaches is unverifiable, and that what it promises for the foreseeable future is more of the same.

GILBERT ALLARDYCE  
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DENIS MACK SMITH. *Mussolini's Roman Empire*. New York: Viking Press. 1976. Pp. xii, 322. \$12.95.

With publications spanning more than two decades, Denis Mack Smith has established himself as a leading authority on the Italian Risorgimento. That he now chooses to write on the Fascist regime is hardly surprising, but the result is perplexing, even infelicitous. Particularly striking is the fact that Mack Smith's assessment of Mussolini has not changed since his brief article on the Duce in *History Today* (April 1959). Although scholars are under no obligation to alter their evaluations, the passage of time certainly ought to favor greater perspective, more incisive understanding, and more interpretative reflections on the origins of the Fascist state in Italy. If, indeed, as many now believe, the roots of Mussolini's movement lay far back in the Italian experience, Mack Smith ought to be singularly well qualified to cast light back down the path of Italian history. This he does not do in any way in this volume.

From its title (certainly ironic—perhaps sarcastic?) to the final chapter, *Mussolini's Roman Empire* is the highly individualistic creation of its author. While defying any simple classification, this volume could be considered primarily an account of Mussolini's foreign policy. Yet that statement is misleading since the meat of the work is an account of Mussolini the master propagandist, the *poseur*, the buffoon who duped the Italians, transfixed foreign governments and their leaders, and who, finally, ingested *in toto* his own lies and brought the whole regime down through his own follies. In short, Mussolini seemingly made Italy a great power when in fact it was little more than a shrewdly promoted Potemkin village. Undeniably there is some truth in this, but it is too simplistic, particularly from this author.

Seventeen chapters make up this book; each might conceivably stand alone as an article. The

period covered is 1922 to 1945, but the issues singled out for discussion are not comprehensive of all the major events of those years. There are two chapters on Italian colonialism, an interesting trio on oscillating relations between Hitler and Mussolini, sections on the Ethiopian War, the Spanish Civil War and, at the end, World War II, albeit in unsatisfactory brevity. Significantly left aside, though, is such a central matter as Mussolini's dealings with the Church. Corporatism is virtually ignored; yet if one wants to speak of Mussolini's success as a propagandist, why not make use of what was probably the biggest hoax he perpetrated?

A somewhat disconcerting aspect of Mack Smith's book is the use of source materials. One cannot escape the initial impression that the job of footnoting was done by an over-zealous undergraduate. Peers hardly expect from Mack Smith some 1,650 footnotes encompassing at least 5,000 citations in such a short book. The questions arise: "Why a note here?" "Why *not* a note there?" Then it becomes more disturbing to find, in the relatively small number of citations that can be checked even in several days, that the author was apparently ill-served by an assistant. Spot-checking reveals, for example, that on the Pact of Steel citations to Ciano's diary are not infrequently incorrect. Did someone mistake *marzo* for *maggio* of 1939? The assertion that on occasion the Fascists contemplated murdering the kings of Greece and Albania is in no way substantiated by the reference to *Documents on German Foreign Policy, 1918-1945* and Rebecca West's *Black Lamb and Grey Falcon*. In many instances, may it be clearly understood, references are wholly accurate, although occasionally some might wish to quarrel with the author's interpretations. Still, the chapter on the armed forces and war preparedness loses credibility as the number of incorrect footnotes increases. The degree of reliance on secondary sources weakens the book as does the quantity of citations to old published materials (e.g., Mussolini's *Opera omnia*). Of new references, virtually the only collection to be consulted was the truly valuable set of papers at St. Antony's College.

No student of modern Italy can afford to ignore any work by Mack Smith; but, if there be any truth in the rumor that this is the first of a two-volume work, may we not hope that the concluding segment will return to the standards we have come to expect from this author?

BENJAMIN F. BROWN  
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PHILIP V. CANNISTRARO. *La fabbrica del consenso: Fascismo e mass media*. Translated by GIOVANNI FERRARA. Preface by RENZO DE FELICE. (Tempi nuovi,

number 74.) Bari: Laterza. 1975. Pp. xiv, 497. L. 3,700.

In this abundantly documented study of the Italian Fascist government's manipulation of the press, cinema, and radio, Philip V. Cannistraro seems to be of two minds concerning the nature and purposes of Italian Fascism. One suspects, also, that he is never completely sure what he means when he uses the word "culture" in his often illuminating and detailed analyses of how the Fascist government (and the neo-Fascist Republic of Salò) attempted to make the principal means of mass communication serve its ends at various moments during the years 1922-44.

Some of the book's ambiguities can be attributed to the apparent influence exerted on Cannistraro by Renzo De Felice, whose effort to be "objective and serene" in his multivolume biography of Mussolini and whose consequent discovery of certain positive and "progressive" aspects of the Fascist experience, have had a notable impact on historians of Fascism, especially in Italy and in the United States. In his preface to this book, De Felice notes with obvious pleasure that Cannistraro refuses to fit the Italian Fascist regime into the "procrustean bed" of an outmoded "totalitarian typology."

Despite his basic agreement with Croce's view of fascism as "anti-culture," and his personal conviction that under Fascism "there was no real distinction between culture and propaganda" (in which case we are not dealing with "culture" but with some other category that ought to have been more sharply defined by the author), Cannistraro tells us that "a constant feature of Fascist cultural policy was the attempt . . . to destroy the traditional class base of Italian culture" (p. 9) and that, through the radio, much more than through the press and cinema, the Fascist regime "broadened the political and cultural consciousness of many Italians who otherwise would have remained isolated and cut off from modern life" (p. 270). It is difficult, if not impossible, to reconcile these and other similar judgments with the author's view of Italian Fascism as "a corrupting and dehumanizing phenomenon" (p. 3), as bent solely on "the political indoctrination of the Italians" (p. 27), as xenophobic in the extreme and committed inexorably to "closing the doors" to all foreign cultures (p. 50), as responsible for a press whose campaign to create a "heroic image" of Mussolini had few equals in its deceitfulness and vulgarity (p. 80). One could cite many other examples of Cannistraro's fundamentally sound grasp of the Italian Fascist phenomenon.

It is this reviewer's considered opinion that it is only when Cannistraro, following De Felice's lead, tries to be impartial and "serene" that he really

distorts not only the nature of Fascism as both a "movement" and as a "regime," but also the meaning of admittedly complex concepts such as "culture," "politics," and "consciousness."

FRANK ROSENGARTEN

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PAOLO POMBENI. *Le "Cronache Sociali" di Dossetti, 1947-1951: Geografia di un movimento di opinione*. Florence: Vallecchi Editore. 1976. Pp. vi, 242. L. 5,500.

After the promising political and social experiences of the Italian resistance, the years following World War II witnessed a disappointing breakdown of the anti-Fascist coalition and the steady entrenchment of the Christian Democratic party (DC), which has dominated Italy's government ever since. But at the very time when the DC was consolidating its power under Alcide De Gasperi, a "left-wing" opposition emerged within the party around the figure of Giuseppe Dossetti. Paolo Pombeni has not attempted to write either a political or ideological history of the Dossetti faction; rather, he has focused on its biweekly journal, *Cronache Sociali*, edited by Giuseppe Glisenti. His book helps to explain the relationship between the DC and *dossettismo*, but only by defining the dimensions and sources of support that the group stimulated.

The value of this book lies in its careful and intelligent treatment of the journal as a case study of the "geography" of a movement of ideas. The author is to be congratulated for having developed and applied a useful methodology to the study of the communication of ideas and the interaction between printed media and audience reception.

After a brief description of the origins and history of *Cronache Sociali*, the book presents four tightly argued chapters of quantitative analysis of the journal's circulation 1.) through subscriptions, 2.) by newsstands and local sponsors, 3.) among Church-affiliated groups, and 4.) within DC and other political circles. Each of these sectors is broken down along regional and urban/rural lines and according to the social-cultural backgrounds of the readers. Although relying largely on the incomplete archives of the editorial and managing staffs, Pombeni has sifted the available evidence cautiously in order to create an audience profile and thereby determine the nature of the journal's appeal.

In a thoughtful concluding chapter, he offers an overall interpretation of the roles of *Cronache Sociali* and *dossettismo* in postwar Italian society. The journal had a diverse but broadly based readership claiming no strong ties either with the DC leaders or the Dossetti faction. Rather, it sought to pro-



voke dialogue among those critical thinking, independent, socially conscious Italian Catholics who recognized the need to deal with the problems of industrialization and the modernization of traditional society. It was most popular where there was little die-hard Church repression, where local Catholics had fought in the resistance and later faced strong working-class parties, and where the pressures of industrial economy were most severe.

In the light of Pombeni's findings, the DC's opposition to *Cronache Sociali* and the Dossettian challenge confirms what we already know about the nature of Italian Christian Democracy. The rigorously sustained thesis, carefully controlled methodology, and suggestive conclusions of this volume make interesting reading in an otherwise depressing subject.

PHILIP V. CANNISTRARO  
Florida State University

*Structure sociale et développement culturel des villes Sud-Est européennes et adriatiques aux XVII<sup>e</sup>-XVIII<sup>e</sup> siècles.* (Actes du Colloque interdisciplinaire organisé par la Commission d'Histoire de la vie économique et sociale dans les Balkans et la Commission d'Histoire des idées dans le Sud-Est européen sous les auspices de la Fondation Giorgio Cini de Venise et du Comité italien de l'AIESEE, tenu à Venise 27-30 mai 1971.) Bucharest: Association Internationale d'Études du Sud-Est Européen. 1975. Pp. 377.

The complete papers, discussion, and remarks of the second meeting of the *Association d'études du Sud-Est européen* appear entirely in Western European languages. Almost five years in publication in Bucharest and aided by UNESCO funds, it records the group's second colloquy (the first took place in Moscow in 1969). Its concern is the cultural development and social structure of the cities of the Balkans and Southeast Europe in the seventeenth and eighteenth centuries. Concentration on the early modern era raises immediate questions: is this a continuation of *le monde Braudellien* or an exercise in Marxist interpretation of urban growth, a study of Islamization or a comment on some of the world's truly ancient cities on the eve of modernization? The ambitious answer is that it is all these, and more, creating considerable confusion, a profusion of historical problems, and an inevitable division among participants over the nature of social structure itself. No accepted definition of urbanity prevailed and no basic concurrence emerged, as the closing statements of the colloquy candidly admit. But this is not all loss: certain of the papers are extremely valuable and scholarship can still profit from the ideological exchange of Marxists and members of the *Annales* school, while communication between scholars familiar with the Ottoman and Arabic sources and

those utilizing Greek and Western sources is essential if our core of knowledge is to increase.

The major difficulty is not a lack of ideological agreement but a deeper dilemma facing Southeast European studies: fitting the unique experience of this small corner of Europe into an historiographical framework designed for and based upon the experience of the West. The historian tends to explain why southeastern cities fail to undergo the changes common in the West, a tendency of all ideological persuasions among the fifty-one participants from fifteen European nations and the United States.

A thoughtful paper by Traian Stoianovich explores why Ragusa (Dubrovnik)—certainly the most Western of Europe's southeastern cities—failed to utilize the printing press in the modern era. The behavior of Western urban centers, most particularly Venice, therefore becomes the measuring rod of typical development; Ragusa's deviation, perforce, is measured by a negative component: its failure to comply. The merit of this approach lies in establishing essential differences between East and West in Europe, but it fails in providing a necessary interpretive framework for the particular urbanity of the area, where the city is both more ancient and more persistent than in the West. Here the city is not strictly medieval in origin; consequently it does not benefit from the interpretive framework of Henri Pirenne and defies part or all of the theoretical extrapolations offered by his successors. Such problems as that of Durazzo in Odile Daniel's paper is typical. A city of the ancient world, still commercially active in the Middle Ages, it was reduced to two hundred souls in the eighteenth century yet maintained some distinctly urban forms. Such a center confounds the growth- and progress-oriented arguments common to the ideologically distinct opinions represented at the colloquy. The participants struggle with this historiographical difficulty. They even make attempts at quantification to establish criteria for urbanity. This raises the additional problem of available source materials; only a few Arabic sources can support attempts at quantification, and aside from that historians have had to rely on foreign, and mostly Western, travelers for estimates of size.

A third difficulty is that urban life has been subject to such vast changes over the centuries in Southeast Europe. At one moment in time a city may come tantalizingly close to the prototype of Henri Pirenne, or Max Weber, or Lewis Mumford, or Karl Marx, only to veer off in its long and varied course into baffling new directions. In certain regions new administrative centers established by the Turks absorbed the urban functions of older neighboring cities, while other regions contained urban centers so tenacious that no effort



of civil authority was able to displace them. An adequate interpretive frame for Southeast European cities must account for this.

The task of interpretation to which this colloquy addressed itself has undeniable merit for the better comprehension of urban culture in the modern world. One small illustration will suffice: the bachelor *Han* of the Islamic city. It provided an entirely male and mobile sources of labor, complementing the labor supply of stable working-class urban families. Undoubtedly it affected the social and economic behavior of lower-class people, and, adopted as a solution for migrant male workers in the cities of Western Europe and of North and South America, it came to influence working-class response in industrialized cities as well. In this respect, at least, the modern Western city is not understood only through investigating its own history but also that of Southeast Europe, from which it imported social structures.

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College at Brockport

WILLIAM J. STILLMAN. *Articles and Despatches from Crete*. Edited with an introduction and notes by GEORGE GEORGIADIS ARNAKIS. (American Interest in the Cretan Struggle, 1866–1869. Volume 2.) Austin, Texas: Center for Neo-Hellenic Studies. 1976. Pp. 138. \$12.00.

William J. Stillman was the American consul at Canea, Crete from 1865 to 1869. He is important not because of his performance of consular duties, which by any standard were minimal, but because he became the ardent champion of the island's Greek Christian population in a revolution that broke out against the Turks in 1866. Stillman himself wrote a book about the conflict, *The Cretan Insurrection of 1866–7–8* (1874), which was edited by George G. Arnakis and republished in 1966 under the title *American Consul in a Cretan War—William J. Stillman* as the first volume in a series called *American Interest in the Cretan Struggle, 1866–1869*.

Before his recent death Arnakis completed a second volume, which also deals with Stillman. After a short introduction, this book includes three major types of materials: 1.) selected dispatches of Stillman's from Crete and Greece, some of which were previously published in *Executive Documents Printed by Order of the House of Representatives during the Second Session of the Thirty-ninth Congress, 1866–67* (1867): Ex. Doc. 38 (January 17, 1867), "Revolution in Candia"; 2.) reprints of articles by Stillman, most of which were written at the time of the Cretan conflict; and 3.) four petitions from the Cretan Christians, including two that were previously published in the *Executive Documents* cited above. All four are given by Arnakis in an appen-

dix along with two letters to Stillman from the American vice-consul at Canea.

To judge from the selection of dispatches and the articles in this book, Stillman—even given his pro-Christian point of view—was a good observer of the day-to-day events that took place on the island, but he was far from successful in understanding Crete in the wider framework of the Eastern Question and the attitudes of the European powers. His references about the policies of the Greek government toward Crete seem sometimes naive, if not incredible coming as they do from a man who should have known the diplomatic scene. He strongly condemned Prime Minister Voulgaris (Boulgaris) for not giving greater assistance to the insurgents in late 1868; but it is hard to see how Voulgaris could have done more without courting disaster in a war with the Ottoman Empire, since Greece was totally unprepared and all the powers were strongly urging moderation. The point is clear: Stillman had so identified himself with the cause of the Cretan Christians that he could only view the Athens government or any other government involved with the island in a highly emotional way.

Certainly, however, the book gives more than just insights into one consul's character and ideas. There is much of interest here for both Cretan history and nineteenth-century American diplomacy. Arnakis brought these materials together in a clear, perceptive manner.

GEORGE J. MARCOPOULOS  
Tufts University

GEORGE B. LEON. *Greece and the Great Powers, 1914–1917*. (Institute for Balkan Studies, Serial, number 143.) Salonika: Institute for Balkan Studies. 1974. Pp. xiv, 521.

Although the bibliography on Greece's turbulent World War I years is extensive, the individual works are generally characterized by limited historical research or polemical, biased interpretations. George Leon's study now emerges as the most valuable and balanced investigation. By examining closely the unpublished materials of the Greek and German foreign ministries and of Great Britain's cabinet, and by utilizing effectively the published—but little used—documents of tsarist Russia's foreign ministry, the author details the intensive diplomatic maneuvering from the war's outbreak to Greece's entrance three years later.

Controversy has centered on the deliberate policy of Eleftherios Venizelos, the Liberal Party leader, to link Greece with the Entente, the determination of King Constantine to maintain his nation's neutrality, and the indirect and subsequently direct involvement of the great powers in Greece's internal politics. The landing of Allied

troops in Salonika in October 1915 and the establishment of Venizelos' provisional government one year later in that port contributed to the growing national division; Greek lined up against Greek.

Despite its professed status of neutrality for nearly three years, Greece had become a battleground on which none of the combatants acted honorably. The blundering yet forceful tactics of the Entente shifted the earlier, predominantly pro-Allied sentiments of the Greek public increasingly toward neutrality during 1916. Consequently, King Constantine and German Balkan strategy benefited. London and Paris, not to be denied, worked closely with their client, Venizelos, to engineer the downfall of Berlin's client, Constantine, who departed Greece in June 1917. The next month, with Venizelos again in Athens as prime minister, the nation joined the Entente. Many of the communications between Constantine and German officials have been destroyed or lost, but enough dispatches remain for Leon to document accurately for the first time their close ties.

Because the presentation of these momentous events is so authoritative, it is regrettable that the book comes to an abrupt halt with the fall of Constantine. The author provides neither a conclusion for his monograph nor a fitting commentary to highlight the impact of Greece's division into hostile camps during 1915-1917—the trauma and enmity of these years influenced the conduct of Greek politics well into the post-World War II era. Notwithstanding the missing historical overview, the details are here in abundance.

S. VICTOR PAPACOSMA  
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ALEXIS AD. KYROU. *Ōneira kai pragmatikotēs: Sarantapente chronia diplōmatikēs zōēs* [Dreams and Reality: Forty-five Years of Diplomatic Life]. Athens: P. Kleisiounes. 1972. Pp. 398.

A Greek foreign service officer for forty-five years, Alexis Kyrrou died one year after his 1968 retirement. These memoirs, published posthumously by his family, cover the first thirty years of a diplomatic career that found him at posts in Istanbul, Nicosia, Berlin, Belgrade, Warsaw, London, and New York.

Kyrrou's recollections provide a succession of chatty vignettes and commentaries on prominent Greek and international personalities, but suffer from too little analysis and few insights into the problems of Greece's foreign policy for the three eventful decades from 1923 to 1953. Thus, despite the significance of Athens' relations with Berlin during his service in the German capital from February 1932 to December 1936, Kyrrou divulges little of value for the historian. A similar judgment must be delivered for the chapter on his subsequent

assignment in Belgrade during the three years preceding the outbreak of World War II—a period that witnessed the breakdown of intra-Balkan agreements. Some worthwhile information, however, can be ferreted out of Kyrrou's last sections dealing with his work on Athens' delegation at the 1946 Paris Peace Conference, his associations with the United Nations Balkan Commission of Inquiry, and his term as Greece's permanent representative to the United Nations from 1947 to 1954.

S. VICTOR PAPACOSMA  
Kent State University

ELINOR MURRAY DESPALATOVIĆ. *Ljudevit Gaj and the Illyrian Movement*. (East European Monographs, number 12.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1975. Pp. vii, 271. \$12.00.

This monograph falls somewhere between a full-length biography of Ljudevit Gaj and a history of the Croatian national awakening in the 1830s and 1840s. Because little has been written in English on either topic, it is a valuable addition to the historiography of nationalism in Eastern Europe. The author has meticulously researched the numerous archival and published materials for her study and has reconstructed the story of Gaj's literary, political, and personal activities in precise detail. The first two chapters contain definitions and a useful outline of the South Slavs' development to about 1830. The remaining ten chapters chronologically trace Gaj's career, which is divided into two parts: to 1842, when Gaj was the principal ideologue of the Illyrian Movement in its literary-cultural phase; and 1843-48, when Gaj's influence diminished and the Movement became a political struggle against the Hungarians.

Elinor Murray Despalatović does not offer novel insights into the nature of the Illyrian Movement but rather clarifies Gaj's role within it. As editor of the journal *Danica* Gaj provided the Croats with the basis for their literary language and the ideology of Pan-Slavic nationalism. Yet he managed both his personal finances and his politics ineptly. Excluded from political leadership by his common birth, he engaged in secret intrigues with Russia, Serbia, and the Polish Emigration. By the 1840s he was in constant debt and had alienated many of the Croatian intellectuals.

Despalatović holds generally to the traditional ideological interpretation of nationalism represented by Kedourie and Kohn, the only authorities she cites in the realm of theory. She omits from her study some larger questions like the relationship of Metternich to the Illyrian Movement. For the most part, however, her prose is clear and effective, her conclusions sound, and her portrait of Gaj,

"the first 'professional' nationalist" among the Croats, skillfully drawn.

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*Augustana College*

FRED SINGLETON. *Twentieth-Century Yugoslavia*. New York: Columbia University Press. 1976. Pp. xiv, 346. \$5.95.

This is a remarkably austere book. Its author attempts a thorough analysis of contemporary Yugoslavia while eschewing—except as *en passant* mentions—such "juicy" episodes as the Tito-Stalin rift; the purge of the Cominformists Žujović, Hebrang, *et al.*; the Djilas crisis; the purge of Ranković; the persecution of Mihailov; etc. Certainly intentional, these omissions appear to be an expression of the author's deliberate self-restraint. This in turn is presumably based on a decision that the above-mentioned headline-making crises have been adequately analyzed elsewhere, and that the interested reader now needs a more structured, albeit prosaic, analysis.

Over the past three decades Fred Singleton has visited Yugoslavia almost annually, and this book gives evidence of his familiarity with the country and his sympathetic, albeit detached, understanding of its political and socioeconomic experiments. It begins with a rapid geographical and historical survey of ninety-nine pages carrying Yugoslavia through World War II, and then presents a deeper analysis of the Yugoslav constitutional and socioeconomic experiments, experiences, successes, and problems in the Titoist era. Here the best chapters are those devoted to such usually dry subjects as the banking system and public investment strategies. The book closes with a characteristically cautious series of future-oriented questions, indicating that in Singleton's judgment it is still premature to pronounce the current Yugoslav system a success. He is emphatically aware that the national question, which eroded the legitimacy of the interwar regime and led to ethnic civil war during World War II, is still unresolved, though it now expresses itself in economic and cultural rather than violent forms. He also asks how deeply the citizens have internalized the official values of socialist humanism. And he wonders how much substance there really is behind the appearance of Yugoslavia's—specifically Tito's—highly vivid and seemingly pivotal international role.

Since the end of World War II the Yugoslav regime has experimented with five major constitutional changes and many additional functional variations in economic planning mechanisms and industrial organization. Within these several frameworks the country has achieved an impres-

sive overall transition from a relatively retarded agrarian society to a relatively advanced industrializing one. The main burden of this industrialization was originally borne, in the 1950s, by the peasantry and later, in the so-called era of market socialism in the 1960s, by the unskilled workers; the latter were eventually enabled to ease their lot by becoming guest-laborers in Western Europe. But one major goal of this otherwise impressive industrialization drive has persistently eluded the Yugoslav regime: the country's underdeveloped southern regions have failed to grow at a faster rate than the national average, and hence the gap between them and the more developed northern ones remains as wide today as it was thirty years ago.

JOSEPH ROTHSCHILD  
*Columbia University*

GEORGE F. JEWSBURY. *The Russian Annexation of Bessarabia, 1774-1828: A Study of Imperial Expansion*. (East European Monographs, number 15.) New York: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. 199. \$12.00.

This monograph deals with the Russian annexation of Bessarabia and its administration up to the outbreak of the Russo-Turkish War in 1828. The subject is of particular interest because Russian control of this predominantly Romanian area has been a major issue in dispute between the two nations since that time. George F. Jewsbury first presents a detailed discussion of the Russian occupation of the Danubian Principalities from 1806 until the withdrawal in 1812. At that time, after the signing of the Treaty of Bucharest, Russia returned the major portion of the Romanian lands to the Ottoman Empire, but kept this Moldavian territory, which, incidentally, was only referred to as Bessarabia following 1814-15.

Thereafter, with the intention of assuring stable conditions in this highly strategic area, the Russian government made a determined effort to introduce effective political institutions conforming to the administrative ideals of enlightened despotism. The objective was to establish a just and efficient administration based on local Romanian customs. Initially no attempt was made to reform the government along Russian lines. This goal was reflected in the administrative statute drawn up by a committee of Russian officials and local boyars and issued in 1818. This document gave Bessarabia more autonomy than any other part of the Russian empire and placed it on a level with Finland and Congress Poland. Jewsbury blames the failure of the subsequent administration directly on the great landowners: "The experiment in local au-

tonomy had failed for a very simple reason. Self-interest was the sole motivating goal of the boyars and they saw in the 1818 Statute no reason to change their way of life . . . (p. 131). National interest was interpreted in Kishinev as the interest of the Romanian boyars, instead of that of the entire population, or that of the province" (132). After 1823 the autonomous rights were gradually curtailed and finally ended in 1828; by 1833 Russian had been made the sole language of administration. This interesting account is an important contribution to the fields of both Russian imperial administration and Romanian national history.

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Bloomington

KARL NEHRING. *Matthias Corvinus, Kaiser Friedrich III, und das Reich: Zum hunyadisch-habsburgischen Gegensatz im Donauraum*. (Südosteuropäische Arbeiten.) Munich: R. Oldenbourg Verlag. 1975. Pp. 244.

Almost ninety years have passed since the publication of the German translation of Bishop Vilmos Fraknói's study of Matthias Corvinus (1458–90), the Renaissance monarch of Hungary. Although a large number of books and articles have appeared on the cultural history of Hungary during the reign of this energetic king, no major new study has been devoted to the diplomatic activities of Matthias Hunyadi. Fraknói's book, with all of its shortcomings, its excessive national and clerical orientation, has remained the major source of information, in a Western language, for the diplomatic history of Matthias' reign. This new study by Karl Nehring will largely replace Fraknói.

Nehring has written an excellent book on the clash between the dynastic interests of the House of Habsburg and the newly rising power of the Hunyadi family in Hungary. His work is of great value and is the result of meticulous scholarship. Written as a doctoral dissertation at the Südost-Institut in Munich, it has all the virtues associated with a dissertation and none of its shortcomings. Based on extensive new archival findings as well as a judicious use of secondary sources, the volume is written with lucidity and objectivity. Nehring shows how the Emperor Frederick III forced Matthias Corvinus in 1463 to accept the Treaty of Wiener Neustadt, in which the Hungarian king recognized the right of the Habsburgs to the Crown of St. Stephen should Matthias die without a legitimate male heir. Until his death in 1490, Corvinus attempted to reverse this situation through diplomacy, military means, and all the considerable craftiness at his command. Frederick III, on the other hand, stood his ground; although

usually outmaneuvered and militarily humiliated by the Hungarian monarch, he managed to survive and outlive his opponent. This, in essence, sealed Hungary's fate and led to its eventual incorporation into the possessions of the House of Habsburg.

Nehring traces the clash between the Hunyadi and Habsburg interests in three chapters and designates his final chapter as a conclusion. This fourth chapter is, however, much more than a summary of the material previously discussed. The author makes an attempt to place Matthias Corvinus in the perspective of a Renaissance prince, and shows how the Hungarian monarch used pomp and ceremony, the cultivation of friendship with humanists, and even his position as a great patron of the arts to buttress and extend his statecraft. A study on Renaissance Hungary, which would give a fuller treatment of the cultural policy of Matthias, to augment Nehring's fine diplomatic history, is yet to be written.

One annoying feature of this study is the constant germanization of all personal and geographic names. Why Bohemian, Polish, and Hungarian place and family names have to be given in German is difficult to understand, although the author does provide a concordance of geographic names in the appendix. The appendix also contains the complete text of documents relating to the Treaty of Wiener Neustadt and an excellent bibliography and index.

Nehring's book is indispensable to all scholars who have a serious interest in the diplomatic history of East Central Europe in the late fifteenth century.

L. S. DOMONKOS  
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PAUL BÖDY. *Joseph Eötvös and the Modernization of Hungary, 1840–1879: A Study of Ideas of Individuality and Social Pluralism in Modern Politics*. (Transactions of the American Philosophical Society. New Series, volume 62, part 2.) Philadelphia: the Society. 1972. Pp. 134. \$5.00.

For almost a century Baron Joseph Eötvös (1812–71) and his works were the preserve of literary scholars. Since the mid-1960s, however, there has been a general upsurge in the number of publications that deal with him as a liberal statesman and political philosopher. Part of this upsurge stems undoubtedly from such relatively recent events as the centennial of the Austro-Hungarian Compromise (1967)—in which Eötvös had a hand—and from the one-hundredth anniversary of his death (1971). But part of it must also be due to Eötvös' attractive personality and liberal political philosophy, which make the study of nineteenth-century Hungarian history—filled as it was with



bitter nationality struggles and mutual recriminations—a much more appealing undertaking.

Paul Bödy's monograph is the first significant American attempt to summarize and to interpret the thoughts and achievements of Eötvös during a period of Hungarian history (1840–70) that encompassed both a major revolutionary (1848–49), as well as a major evolutionary or compromising effort (1867) in the direction of national salvation. The best part of his work is undoubtedly the chapter on Eötvös' critique of nationalism (chapter 5) wherein Bödy analyzes Eötvös' views and proposals for the reconstruction of Hungary, and does so within a broader European context (e.g. in comparison with the ideas of Tocqueville, Sismondi, Guizot, Mill, and Palacký). Its weakest point is his lack of effort or desire to deal with some of Eötvös' significant literary works as reflections of his political views and as tools in his struggle for the transformation of Hungary "into a modern, liberal, and cultured society" (p. 125).

All in all, Bödy's monograph is an admirable work, based on considerable research and executed with impeccable scholarship. But Eötvös still merits a more extensive treatment in a more accessible form, and I can only encourage Bödy to continue his writing on this deserving topic.

STEVEN BELA VARDY  
*Duquesne University*

L. TILKOVSKY. *Pál Teleki (1879–1941): A Biographical Sketch*. Edited by F. MUCSI. Translated by D. SZÉKELY. Translation revised by I. GOMBOS. (*Studia Historica Academiae Scientiarum Hungaricae*, number 86.) Budapest: Akadémiai Kiadó. 1974. Pp. 70. \$4.50.

Not counting Regent Horthy and perhaps one or two others, Count Pál Teleki was undoubtedly interwar Hungary's most significant statesman. His stature, however, rested at least as much on his position in the country's scholarly and cultural life, and on his own scholarly reputation, as on his two prime ministerships (1920–21 and 1939–41).

As an outstanding representative of the Transylvanian-Hungarian aristocracy, known for their relative progressivism, Teleki too was basically a progressive man. Moreover, like the great nineteenth-century philosopher-statesman Joseph Eötvös, Teleki was also a conscious "European." This did not make him less of a Hungarian patriot. It did, however, prevent him from seeking an accommodation with Soviet Russia, and made it impossible for him to find a real common platform with National Socialist Germany—notwithstanding the two countries' common interest in revisionism. It was this "Europeanism" in Teleki that made him turn against the Radical Right and ultimately obliged him to take his own life.

In light of his significance, Teleki should have found a biographer long ago. That he did not can only be attributed to his anti-Soviet stance. As a universally respected man it would have been difficult to denounce him, but as an anti-Soviet statesman it would have been impossible to praise him in Hungary.

L. Tilkovszky's "biographical sketch," is the first attempt by a Hungarian historian to assess the man as a whole. While I value this attempt, unfortunately I cannot agree with many of the author's conclusions, for he makes Teleki into what he was not: a kind of racist and a "genuine reactionary." Even Teleki's suicide—which was universally interpreted as a protest against the heavy-handed Nazi-German pressure on Hungary to join in the attack against Yugoslavia—is made simply into an "admission of guilt." Teleki may have been attached to an outdated code of honor, but he was an honorable and upright man. Let us hope that the final version of Tilkovszky's biography of Teleki will put this deserving scholar-statesman into a proper light.

STEVEN BELA VARDY  
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A. H. HERMANN. *A History of the Czechs*. London: Allen Lane; distributed by Rowman and Littlefield, Totowa, N.J. 1976. Pp. 324. \$12.00.

This well-written, provocative essay, made up of twenty-one little essays, should go on the reading lists of all students of the Czechs and their history. A. H. Hermann provides fresh insights and challenges on everything in Czech history from Samo to Jan Palach. Bohemia is well placed in the larger context of Central Europe and, to a limited degree, in the still larger sphere of Western Europe. The author, a native Czech with a law degree from Charles University and not a historian, has spent many years in England as a journalist.

His survey is first-rate journalism (in the best sense of the word) and well balanced. It offers social and cultural history as well as the usual overworked political and diplomatic developments. It would be hard to imagine a better fifty-six-page summary of Bohemia from 400 B.C. to White Mountain. The magnificently complicated medieval period of Czech history is especially clearly presented. At the other end of the chronological scale is a welcome chapter on Czech Communists during the 1920s.

The author is particularly good with bold generalizations, summaries, pithy images, insights, and telling parallels between past and present. Although he adds few new facts for scholars, his treatment of well-known events merits reading by specialists and generalists alike. Hermann updates and surpasses the earlier English-language surveys



of Lützow, Monroe, and Nosek, for example, and, while inferior in scholarship to Seton-Watson, this volume is certainly more readable.

Other than the fact that the Slovaks receive but one short chapter, they will find little to complain of in this book which, after all, is a history of the Czechs, not the Czechoslovaks or the Czechs and Slovaks.

This excellent book is ideal for introducing students and laymen to the excitement of the Czech experience and to the importance of the Czech political and cultural role in European history and for guiding them easily through a very complicated part of the history of mankind.

Perhaps better titled "Reflections on Czech History," this volume has four excellent maps, but, alas, no footnotes, no bibliography, and no documentation of any kind.

STANLEY B. KIMBALL  
Southern Illinois University,  
Edwardsville

J. M. KIRSCHBAUM. *Slovak Language and Literature: Essays*. (Readings in Slavic Literatures, number 12.) Cleveland: University of Manitoba. 1975. Pp. xv, 336.

This is a good and interesting book. Until someone writes a better monograph in English on Slovak language and literature, this collection of essays by J. M. Kirschbaum will remain helpful to historians who do not read Slovak. These eleven essays, all written between 1952 and 1974, have either been published in Canada or read at conferences. Although rewritten for this compilation, they are basically products of the late 1950s and early 1960s.

The contributions cover the period from Cyril and Methodius to post-World War II Slovak émigré literature with one big and startling lacuna between Štúr and 1945. Since Kirschbaum is more of a historian than linguist, this book, which is lacking in original analysis and any evaluation of literary merit, is of much more value to historians than linguists.

The individual chapters are well written and documented and, although the author has long held Slovak separatist views, his work is singularly *sine ira et studio*; it could have so easily been just another Slovak attack on the Czechs. His arguments regarding a centuries-old separate and distinct Slovak language and literature are convincing. There is a twenty-four-page French summary (which is becoming obligatory in Canada), a bibliography, index, and twelve illustrations. The volume is visually marred somewhat by lines between the letters, probably caused by too

much press pressure. I recommend the book highly.

STANLEY B. KIMBALL  
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OLGA A. NARKIEWICZ. *The Green Flag: Polish Populist Politics, 1867-1970*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 313. \$17.50.

The author has attempted a historical and politico-scientific analysis of the Polish Populist movement from its beginning until 1970, with some projections into the future. "Stimulating further research into Populism in general, and Polish Populism in particular" is her second aim.

In eleven chapters, major issues covered include: historical background of the Populist movement; a socioeconomic characterization of Polish peasants; and their economic organizations and political forces that stimulated the peasant movement and led to the organization of peasant or Populist parties. "The political intrigues during the war" (World War I) are followed by discussions of Wincenty Witos' government in 1920 and the Polish-Bolshevik war. Land reform, "Populism and Piłsudski," "Front Morges," and Populism during World War II and after constitute a large portion of further chapters. Chapter twelve gives readers a synopsis of "What is Populism" and what is Polish Populism. A bibliography, glossary, and index supplement the contents.

Olga A. Narkiewicz has collected many interesting observations from broad readings, mostly in works published by Communist-sponsored institutions in Poland. Her use of these materials is often arbitrary, and quotations seem to be inserted as a "scholarly apparatus" in supporting categorical contentions and judgments. In many places it is extremely difficult to see any connection among sparkling but quite incoherent and unconvincing observations. This can be illustrated by one statement: "However, he [Piłsudski] was not an inalienable enemy of the Bolsheviks; and indeed, had he and Lenin been able to meet in 1918, after the Brest Litovsk Treaty it is possible that they would have been able to arrange matters peacefully" (p. 155).

One thesis of the book is that Polish Populism should be approached from the position of utilitarian materialism. Its three streams are described as Socialist, Christian Democrat, and economic-nationalist, with some variations. "Mikołajczyk was the first populist leader who synthesized in himself all the three trends . . . . It was this attempt at synthesis which proved so unsuccessful

and made Mikołajczyk flee the country . . ." (p. 283).

The author has left out whole areas, for example, programs and action for social and cultural emancipation of peasants and peasant cooperation in the International Peasant Union. The identification of peasant parties with parliamentary democracy is not properly assessed. Direction of peasant movements and parties toward national objectives was an essential element of their existence but has escaped the author's attention. Polish independence has remained an objective of the Polish peasant movement until now, but this is not reflected in the book. Disregarding the peasant movement during World War II warps all other issues. She misrepresents the role of peasants during that war and disregards various existing publications. In particular, Cornell Research Papers in International Studies, especially *Peasantry in Revolution* by Mehmet Beqiraj and *European Ideologies* edited by Feliks Gross, could provide the author with a firmer point of departure for her bold contentions and might have saved her from arbitrariness of judgment. Many misprints should have been avoided. The book probably does show that peasant movements in East Central Europe offer a serious challenge and should attract more attention of historians, sociologists, and political scientists.

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Stevens Point

ZBIGNIEW MACHALIŃSKI. *Gospodarcza myśl morską II Rzeczypospolitej (1919–1939)* [Maritime Economic Thought in the Second Republic, 1919–1939]. Summary in English. Wrocław: Ossolineum. 1975. Pp. 352. 62 Zł.

Historians have never doubted the importance of Poland's access to the Baltic Sea, not only for economic reasons but also as a pivotal element within the context of her political independence. This fact was recognized by President Woodrow Wilson in his Fourteen Points of January 8, 1918, though Zbigniew Machaliński failed to mention it. Instead, the author begins his detailed study of the theoretical concept of Poland's maritime question with February 10, 1920, when Polish forces reached the Baltic, thereby making the recently reborn republic a maritime state with all ensuing economic and political consequences.

Among the Poles, the idea of becoming a maritime nation was raised for the first time in the monthly *Bandera Polska* (May 1919). The article stressed unmistakably that, "reborn Poland cannot neglect her waters. The significance of water is underscored by the demand for a Polish sea by the

whole of society. Access means a window to the world; it also involves the condition for an independent economy" (p. 12). Yet, economic independence was identified with political sovereignty, and exactly this interdependence of the maritime concept represents the main theme of Machaliński's book.

The study does not discuss maritime policy specifically but offers almost exclusively a chronological development of thought, concepts encompassing different formulations, views, and interpretations by various social and political groups. To this end, the author explores the following problems: 1.) the formation of maritime thought in respect of general stipulations of the policy; 2.) the main concepts of Poland's port policy from 1919 to 1939, affecting not only the policy toward Gdańsk (Danzig) but also the decision to build the harbor of Gdynia; and 3.) the policy on the question of developing a merchant fleet and a requisite ship-building industry. As a result, Gdynia became the central issue in striving for Poland's independent foreign trade, an aim that dominated the decisions of all Polish governments of that period. Concern for the fate of Gdynia and access to the Baltic Sea was implicitly guiding the formulation of Poland's foreign policy, particularly toward Germany.

The author concludes that the government's maritime economic policy met the needs, interests, and support of the whole of Polish society, presenting a unified front on this vital issue, which was effectively used for propaganda purposes.

Remarkably, this study, written in traditional scholarly fashion, is free of Marxian historical dialectic. Because of its pioneering nature, the book is difficult to judge against other works. The bibliography, however, includes documents, monographs, and articles and is very comprehensive—if limited to Polish literature exclusively. It also contains short summaries in English and Russian in addition to an index of persons.

This reviewer welcomes Machaliński's study, which contributes to a better understanding of Polish economic policy and its difficulties and achievements. American research libraries as well as advanced students of Poland's history and economy should take note of this work.

STEPHAN M. HORAK  
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JERZY KRASUSKI. *Stosunki polsko-niemieckie, 1919–1932* [Polish-German Relations, 1919–1932]. (Studium Niemcoznawcze Instytutu Zachodniego, number 28.) 2d ed. Poznań: Instytut Zachodni. 1975. Pp. 467. 100 Zł.

Jerzy Krasuski, an independent research scholar in the Institute for Western Affairs, Poznań, is the

leading Polish specialist on Polish-German relations in the period 1919–32. The present work is a new, revised, and updated edition of his two-volume study published in Poznań in 1962 and 1964. As he tells us in the preface, it is also a considerably shortened edition, mainly because he omits a great deal of material on German internal affairs, which he treated in his 1969 book on the history of the Reich, 1871–1945.

Although an American historian has meanwhile published a work on the same subject (Harald von Riekhoff, *German-Polish Relations 1918–1939*), Krasuski's work will surely be welcome, particularly to Polish scholars. Some sections have been reorganized and beneficially shortened or extended, especially that on the Polish-German tariff war. Unlike the first edition, the second has an index of persons, but a subject index also would have been an advantage. There is no doubt that this book will be the standard Polish study in this field for some time to come.

There are, of course, some disappointments. The author makes very little use of the *Akten zur deutschen auswärtigen Politik*, Series B., seven volumes of which appeared between 1966 and 1974, covering the period December 1925–December 1927. It is also surprising that he did not work in the archives of the German Foreign Ministry in Bonn. While access to these archives has sometimes been difficult for Polish scholars, this situation has improved in the last six years, and some had used them even before that time, such as Marian Wojciechowski in his study of Polish-German relations in the period 1933–38 (*Stosunki Polsko-Niemieckie, 1933–1938*). Nor has Krasuski consulted the Foreign Office files in the Public Record Office, London, open for this period since 1968, or the files of the French Ministry of Foreign Affairs, opened in 1971 for the period 1918–39. Again, as far as published documents are concerned, he has made no use of the *Documents on British Foreign Policy*, Series I.A., the first five volumes of which, published between 1966 and 1973, cover the period October 1925–December 1928 and contain much significant material on the Polish-German problem. As in the previous edition, there is no chapter on the Free City of Danzig, although it reflected German policy toward Poland or, speaking more precisely, coordinated its policy with that of Berlin.

In sum, while a case can no doubt be made for studying Polish-German relations in the narrow sense, a wider international context is necessary to understand their nature and their place in European politics. This context is very sketchy in Krasuski's book and is to be sought rather in von Riekhoff's. Despite all these caveats, however, this new edition is still very useful and informative for

students of Polish-German relations in the years 1919–32.

ANNA M. CIENCIALA  
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PHILIP F. CANNISTRARO *et al.*, editors. *Poland and the Coming of the Second World War: The Diplomatic Papers of A. J. Drexel Biddle, Jr., United States Ambassador to Poland, 1937–1939*. Columbus: Ohio State University Press. 1976. Pp. xi, 358. \$17.50.

Advising Roosevelt in 1935 about a new ambassador to Poland, William Bullitt wrote: "The man you send to Warsaw must know French and, if possible, should know German, and if you want to get any information from Warsaw, should also be very much of a gentleman and acutely intelligent." Anthony J. Drexel Biddle, the last American ambassador to interwar Poland, satisfied most of these stiff requirements. He served in Warsaw with great distinction from 1937 to the end of the invasion of Poland in 1939, earning the sympathy of Poles and the trust of foreign minister Józef Beck. In fact, Biddle was one of the few diplomats who was on excellent terms with Beck and received the latter's confidences.

The publication of Biddle's dispatches and memoranda preserved in the Roosevelt Library of Hyde Park is a most welcome event. Like those of the first American envoy to Poland, Hugh Gibson—which will be published in the near future—these papers are an important source for American-Polish relations and indeed for a picture of Poland as seen by a keen Western observer. They also contribute to our understanding of United States diplomacy on the eve of the Second World War.

The editors have done on the whole a good job. They organized the volume into three parts: a sizeable introduction, Biddle's long report on the origins of the war and the Polish campaign in 1939, and a selection of the ambassador's dispatches in the years 1937–39. Copious annotations explain and clarify some obscure passages in the documents. While there is no bibliography, the footnotes contain many references to pertinent studies on this period. A short index and a map that indicates Biddle's route out of Poland in 1939 complete the book.

What are the book's weaknesses? The editors have failed to avoid a number of minor slips that appear in the introduction, footnotes, index, and on the map. The usefulness of the map particularly is marred by misspellings and serious errors of geography. Spelling mistakes and inconsistencies concerning first names are very evident in the index. Polish and Czech diacritical marks are often wrongly used or are missing altogether. The Czech

form of Teschen, for example, is almost unrecognizable. To cite a few small factual errors: Arciszewski was never officially vice-minister; Łepkowski was head of the president's civil chancellery—the function of “Counselor to the President” was nonexistent—Biddle surely could not have been ambassador to Norway since the United States had only a legation and not an embassy in Oslo.

While one could wish for more careful editing, these trifling mistakes, as irritating as they are, do not detract from the value of the book. The publication of Biddle's reports to Roosevelt is an important addition to existing printed documentary sources, and diplomatic historians will appreciate the appearance of this noteworthy volume.

EDWARD D. WYNOT, JR.  
Florida State University

EMANUEL RINGELBLUM. *Polish-Jewish Relations during the Second World War*. Edited and with footnotes by JOSEPH KERMISH and SHMUEL KRAKOWSKI. Translated by DANA ALLON *et al.* New York: Howard Fertig. 1976. Pp. xxxix, 330. \$18.50.

The relationship between Polish Christians and Jews during World War II has long absorbed historians and publicists alike. The result has been an impressive number of monographic, autobiographical, and journalistic studies, with a predictable variance in quality. Alongside Emanuel Ringelblum's contemporary analysis, these other works recede into insignificance. Written during the difficult times following the final destruction of the Warsaw Ghetto in 1943, the study draws on the wealth of documentary material collected by the underground Jewish archives, *Oneg Shabbat*, as well as on the author's personal observations and information sources. Excellent editorial notes and commentary by Joseph Kermish and Shmuel Krakowski enhance the value of the book by updating and verifying Ringelblum's findings.

The book is divided along both chronological and topical lines. Together with chapters that examine the Christian attitude toward the Jews at each stage of the prewar, war, and occupation experiences, Ringelblum also offers separate sections dealing with such subjects as the economic aspect of the Polish-Jewish relationship, the situation of Jewish adults and children who managed to escape from the ghetto to the “Aryan side,” and the posture of Polish social strata, political parties, and government circles toward the Jews. Throughout the narrative run two general themes: the spread of a vicious anti-Semitic mentality from its prewar confines among right-wing political circles to encompass most of the Polish population during the war, and the rare courage of certain Poles who

braved the scorn, hostility, and occasionally the active opposition of anti-Semitic neighbors as well as German occupiers to assist the beleaguered Jews. The author rightly condemns the former trend and extols the latter, in both cases supplying precise, detailed examples of events and personalities to support his case. Since the work was halted prematurely with the author's execution by the Gestapo in March 1944, the two editors have supplied documentary evidence to bolster and elaborate upon Ringelblum's findings. The prevailing picture of daily life spent in terror, of ceaseless struggle for bare survival against overwhelming odds, is effective testimony to both the enormous capacity for human endurance and the equally expansive possibilities of mankind's potential for inhumanity.

EDWARD D. WYNOT, JR.  
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PERTTI LUNTINEN. *The Baltic Question, 1903–1908*. (Suomalaisen Tiedeakatemian Toimituksia, Annales Academiae Scientiarum Fennicae, Sarja, Ser. Bnide, volume 195.) Helsinki: Suomalainen Tiedeakatemia. 1975. Pp. 252. 60 MK.

The Finnish Academy of Sciences had already published a number of historical studies in English, German, and French, thus performing a great service not only to the Finnish scholars but also to Western readers who can now become directly acquainted with Finnish contributions to the discipline. Most of these works are of great value, especially in areas where our knowledge is limited or the available sources and studies are quite meager.

Pertti Luntinen has chosen one such area. His subject matter is an agreement signed by Denmark, Germany, Russia, and Sweden in 1908 pledging to maintain the territorial status quo in the Baltic area. This agreement had induced Great Britain and France to abrogate an older treaty, born during the Crimean War in 1855, which had guaranteed Sweden and Norway against Russia. To a general student of history Luntinen's topic might seem of little if any importance, but this study that covers a series of events leading to the agreement in 1908 clearly demonstrates even to a casual student the great complexity of the so-called Baltic Question. It involved not only the Scandinavian and Baltic peoples, but also vital interests of Germany, Russia, Great Britain, and, to a lesser extent, France. The fate of the Baltic area and Northern Europe depended much on general relations between Germany, Russia, and Great Britain. The rise of independent Norway, the Danish Straits, the provinces of Schleswig and Holstein, the Åland Islands, the Russo-Japanese

War, the Revolution of 1905 in Russia and the Baltic countries, the building of the German High Seas Fleet and the Kiel Canal, British trade interests, and the principles of freedom of seas and the balance of power, along with a number of other items, come into focus in Luntinen's detailed and meticulous study.

For the preparation of this study he has not only read all pertinent books and articles in English, German, Swedish, Finnish, French, and Russian, but has also utilized unpublished sources in British, German, Austrian, and Russian archives. Despite its division into numerous subchapters covering simultaneous events in various countries, Luntinen's book reads quite well. Only a few, not very serious, linguistic errors here and there sometimes distract readers' attention. Luntinen's style is lively, his analyses are bold and well documented, and in his commentaries one can also detect a dash of humor. The Baltic question is still alive today, giving Luntinen's book an additional value in our contemporary world.

EDGAR ANDERSON  
San José State University

A. I. TURGENEV, compiler. *Akty istoricheskie, otnosiashchiesia k Rossii, izvlechennye iz inostrannykh arkhivov i bibliotek*. Volume 1. *Vybiski iz vatikanskogo arkhiva i iz drugikh rimskikh bibliotek i arkhivov, s 1075 po 1584 god* [Documents Relating to Russia, Extracted from Foreign Archives and Libraries. Volume 1, Extracts from Vatican and other Roman Archives and Libraries from 1075 through 1584]. Reprint of 1841 edition. West Vancouver, B.C.: Herculaneum Press. 1973. Pp. xv, 399, xxii. \$35.00.

Here is a most welcome reprint, consisting of 255 documents, many of which are in Latin, a few in Italian and Polish, and all prefaced with summary descriptive titles in Russian. Most come from Vatican sources and reflect the interest the popes showed in central Europe, with special reference to Muscovy, and above all their persistent and persistently aborted desire to achieve church union with Orthodoxy.

The first document is a communication from Pope Gregory VII to Grand Prince Iziaslav in 1075, a reminder of how extensive contacts between Kiev and the West were at the time. Then chronological gaps occur, followed by clusters of documents: a concentration from the first half of the thirteenth century reveals the papacy's role in the establishment of the military Orders in Livonia and Prussia; another group from the early fourteenth century discusses ecclesiastical governance and politics in the Baltic realms. The fifteenth century is sparsely represented.

In the sixteenth century materials become more variegated and abundant. The Livonian War focused the West's attention upon Moscow, and

Ivan the Terrible's appeal to Pope Gregory XIII in 1580 to mediate the conflict revived hopes of church union and led to a flurry of activity in papal chanceries everywhere, culminating in the mission of the Jesuit Antonio Possevino, which is comprehensively reported.

These are no dry decretals. Many aspects of life in central Europe come vividly alive in these communications, particularly in the later documents. Let us hope that the Herculaneum Press will soon issue the *Supplementum*, which originally appeared in 1848.

One thing is disturbing about the reissue of this important and valuable tome: it reminds us how little has been done in compiling external source materials for Russian history since A. I. Turgenev blazed the trail more than 130 years ago.

HUGH F. GRAHAM  
California State College,  
Bakersfield

V. F. ZYBKOVETS. *Natsionalizatsiia monastyrskikh imushchestv v sovetskoi Rossii (1917-1921 gg.)* [The Nationalization of Monastery Property in Soviet Russia, 1917-1921]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 202. 71 k.

V. F. Zybkovets has been writing on Orthodoxy and the sociology of religion for almost fifty years. In this work he has turned his attention to the fate of Orthodox monasteries in the RSFSR from 1917 to 1921.

The monograph contains an essay and appendixes. The former has a bibliographical "introduction"; a socio-economic-political description of the monasteries on the eve of the revolution; a chapter on "The Premises and Conditions of the Nationalization of Monastery Estates," in which the author discusses the early legislation and activities of Soviet authorities vis-à-vis the Church and its monasteries; a long, boring, and repetitious section on "The Nationalization of Monastery Estates in Different Zones of the Country"; a chapter on "Sovkhozy, Communes and Artels Created on the Base of the Monasteries"; and a short conclusion. Of the 92 pages of appendixes, 86 are devoted to a listing of Orthodox monasteries in the Russian Empire in 1917.

The author's themes are, first, that there are not sufficient data to write a definitive study of this type; second, that weaknesses in early Soviet administration (combined with the turmoil of civil war) slowed the pace of nationalization; third, that the Church and the monasteries used all stratagems to resist, often reorganizing themselves into "laboring communes" and continuing to till the same soil under the auspices of Soviet law; and, fourth, that while the nationalization was "on the



whole" achieved by 1921, the battle had not been won entirely.

The author's only possible scholarly contribution is to move up by several months the formation of the first "monastery" commune. But, even if correct, this hardly seems to justify a tendentious work based mostly on previously published materials. On the other hand, as a brief guide to early Soviet policies, and for a listing of the monasteries, the book may have some informational value.

SAMUEL A. OPPENHEIM  
California State College,  
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VASILII N. BERKH. *A Chronological History of the Discovery of the Aleutian Islands or The Exploits of Russian Merchants: With a Supplement of Historical Data on the Fur Trade*. Edited by RICHARD A. PIERCE. Translated by DMITRI KRENOV. (Materials for the Study of Alaska History, number 5.) Kingston, Ontario: Limestone Press. 1974. Pp. x, 127. \$6.00.

RAISA V. MAKAROVA. *Russians on the Pacific, 1743-1799*. Translated and edited by RICHARD A. PIERCE and ALTON S. DONNELLY. (Materials for the Study of Alaska History, number 6.) Kingston, Ontario: Limestone Press. 1975. Pp. vii, 301. \$9.50.

These books are translations of one of the earliest and one of the latest Russian accounts of the activities of the Russian fur traders in the Northwest Pacific. Vasilii N. Berkh's study was first published in 1823 and Raisa V. Markarova's in 1968, but this is the first time that either has been published in English. Richard A. Pierce and Alton S. Donnelly, editors of the series *Materials for the Study of Alaska History*, are to be commended for making them available to the non-Russian specialist and thereby helping to stimulate the resurgence of interest in this area.

Both Berkh and Makarova concentrated primarily on merchant activities from 1743 to 1799, the year in which the Russian-American Company received its charter and a twenty-year monopoly of the Pacific fur trade. Berkh, however, included data on the furs obtained by the Russian-American Company through 1822 and those obtained by the Hudson Bay and Canadian Fur Companies from 1806 to 1822.

Berkh, a graduate of the Imperial Naval Academy, became interested in the activities of the Russian merchants when he sailed to present-day Alaska during the course of Russia's first round-the-world voyage, 1803-06. Unlike most Russian naval officers, who regarded the lowly merchant with contempt, Berkh obviously admired his "enterprising countrymen, whose lack of knowledge was compensated by their boldness and determination." In writing his account he did not depend

solely on his personal experience and information that he gathered firsthand, but zealously collected all the available sources. Having found a sympathetic chronicler, the merchants cooperated by sending him all the information and documents they could find. Unfortunately, many of the merchants' papers had already been lost or destroyed. Berkh's purpose in publishing what he referred to as "this incomplete research" was twofold: first, to honor the "enterprising spirit of the Russian merchant class"; and second, to inspire their descendants "to seek out the papers they possess." Not only was the second purpose not fulfilled, but many of the sources that Berkh used are no longer extant. Much of what was known, prior to Makarova's work, about the Russian fur trade in the Pacific—how the partnerships were formed and the ships outfitted, who sailed where and what islands were discovered, the dangers encountered and the losses suffered, and the number and value of the furs obtained—was contained in Berkh's brief but fact-filled study. As Makarova noted, Berkh's book "contains valuable information about the fur trade not to be found in books written before and after it." Makarova's study does not replace Berkh's but supplements it; she has put the flesh on his skeleton.

*Russians on the Pacific* is based on extensive and thorough research in the Soviet archives. Makarova's 30-page introduction is a comprehensive bibliographical essay, the footnotes provide an excellent, detailed guide to Soviet archival sources, and the bibliography includes non-Russian as well as Russian primary and secondary sources. The appendix contains chronological tables (similar to and based in part on Berkh's) of the Russian voyages from 1743 to 1797 that list the name of each vessel, its "skipper," and owners; the length of the voyage and the islands visited; and the ruble value of the cargo. In addition, Makarova has included the full text (thirty-seven pages) of one of the few extant contracts that define in detail the organization of a company of merchants to engage in the Pacific fur trade. This contract, together with the chapter on the organization of the fur trade, fills an important gap in our knowledge of Russian commercial practices. Makarova's study is an extremely valuable contribution, although her interpretation is strictly Marxist-Leninist.

The translations, on the whole, are accurate, and the editors have included appropriate pictures, maps, and indexes that were not found in the original publications.

MARY E. WHEELER  
North Carolina State University

GLYNN BARRATT. *M. S. Lunin: Catholic Decembrist*. (Slavistic Printings and Reprintings, number 272.) The Hague: Mouton. 1976. Pp. x, 137. \$16.40.

Glynn Barratt continues his work on the Decembrist revolutionaries with a biography of M. S. Lunin, a minor figure among them. Basing his research primarily on published documents (Lunin's diary and letters; the records of the 1826 investigating commission), Barratt reveals Lunin as a man of integrity who consistently and courageously defended his liberal creed even though he recognized the disadvantages in doing so.

This book sheds light on several mysteries about Lunin without completely solving them. To explain Lunin's ardent Catholicism, Barratt suggests the influence of a Catholic tutor in his youth, a Jesuit priest in Paris, and a sojourn in Warsaw after 1822. The author confirms the tenuousness of Lunin's Decembrist connections, for Lunin disapproved of the revolution, did not participate, and was probably (though not certainly) out of touch with its perpetrators after his move to Warsaw. Barratt is less successful in defending his thesis that Lunin's Catholicism and liberalism were all of a piece.

Unfortunately the book is seriously flawed. The prose is often turgid and complicated ("No less than his last years proved, his death was terrible" [p. xi]). Time often gets badly out of joint. The author provides no general conclusion. The manuscript was never proofread (the worst of many errors: "War, every mess acknowledge [sic], was imminent" [p. 8]). In short, the entire book seems a hurried patchwork job. Barratt knows his subject and has ideas to offer. It is a pity he did not take more care in putting them together.

DEBORAH HARDY  
University of Wyoming

DANIEL FIELD. *The End of Serfdom: Nobility and Bureaucracy in Russia, 1855-1861*. (Russian Research Center Studies, number 75.) Cambridge, Mass.: Harvard University Press. 1976. Pp. viii, 472. \$17.50.

Russian autocrats from the reign of Catherine II onward wished to end serfdom, but they always drew back in fear that any change might incite the serfowners, the peasantry, or both, and bring down the autocracy. It is no wonder that early in his reign Alexander II sought to reassure the Moscow nobility that he had no intention of altering the status of the serfs, but, paradoxically, it was this reassurance with its sensitivity to the tensions that existed in the serf system and the statement that reform from above was better than that from below that led to emancipation. By late fall of 1857, the emperor's modest plea for the "amelioration" of the status of the serf was transformed into a project for their liberation.

Daniel Field has provided us with a very important book which will serve as a point of departure for future historians. It is not, however, a definitive

work; it leaves too many important questions unanswered, not the least of which is the motivation of Alexander and Rostovtsev, his most important collaborator, in making their commitment to emancipate the serfs. One also must deplore the unusually large number of misspellings which seriously mar the text. One suspects that a non-Russian, August von Haxthausen, may prove to have been more important than Field allows; the author observes that Haxthausen's views did not differ substantially from those already current in Russia, but he provided Alexander with a *conservative* reason for emancipation, and it was the preservation of the autocracy that was always uppermost in the emperor's mind. Moreover, the author portrays the reformers as faceless bureaucrats, whose deeds are recorded but whose intellects and passions are not.

The author is at his best when dealing with the *pomeshchiki* and their spokesmen. These members of a class being asked to surrender both privilege and economic advantage emerge as more comprehensible personalities. Educated in the traditions of Western moral philosophy, serfowners could not defend the institution of serfdom itself. Moreover, they lacked the opportunity open to their counterparts in the American South of masking their resistance behind a high principle such as states' rights. From the beginning, they had to accede to emancipation although some chose to fight rearguard actions against its economic, legal, and social consequences. The scattered and divided nobility never really had a chance to object to the actions of a government which freely employed dissimulation, intimidation, and deception in its consultations with the provinces. The government read this response as concurrence with its intentions, and ignored such dissent as did occur. The ends were noble, but the means were at times questionable.

The merits of this work are substantial, but we must learn more about the reasons why key individuals acted the way they did if we are to understand fully why emancipation came when it did and under the terms it did. Field amply demonstrates that the reformers had a constituency of one—the emperor—and that matters moved in accordance with his wishes and did not move without his sanction. Now we have an excellent framework within which to ask why.

FORREST A. MILLER  
Vanderbilt University

BORIS SAPIR, edited, annotated, and introduced by. *Lavrov: Years of Emigration: Letters and Documents* [in Russian]. Volume 1, *Lavrov and Lopatin (Correspondence 1870-1883)*; volume 2, *From "Vpered!" to the Group of the Old Narodovol'tsy*. (Russian Series on Social History, number 2. Issued by the Inter-

national Instituut voor Sociale Geschiedenis.) Boston: D. Reidel. 1974. Pp. lx, 603; xvi, 669. \$125.00 the set.

These two large volumes constitute the second major publication of the Russian Series on Social History of the International Institute of Social History in Amsterdam. They make an important contribution to our knowledge, not only of Peter Lavrov, but of a number of other figures in the world of Russian and European socialism in the last third of the nineteenth century. The documents contained in these volumes amplify—although they do not greatly alter—our understanding of several crucial events and episodes, from the heyday of Russian Populism in the 1870s to the renaissance of Russian radicalism twenty years later. Unfortunately, the extraordinary price of these books will prevent all but the most fortunate members of the professoriate from actually acquiring them.

Almost the entire first volume is devoted to the correspondence between Peter Lavrov and German (Herman) Lopatin over the thirteen-year period beginning in 1870. Lopatin was the great adventurer of Populism, and he was to pay for his love of secret missions and daring escapes with more than twenty years in tsarist prisons. One of the first of the notable adventures in which Lopatin was involved was the escape of Lavrov from exile in 1870. Lavrov had been arrested in the general roundup which followed Karakozov's attempt to assassinate Alexander II in 1866, and although no evidence was produced to link him with any revolutionary group, he was exiled to a small town five hundred kilometers north of Moscow. During his exile, Lavrov wrote a series of articles which appeared in book form in 1870 under the title of *Historical Letters*. Lavrov's involvement in the radical activity of the 1860s had been peripheral, but the *Historical Letters* had an impact on the young critics of the regime comparable, for a time, to Chernyshevsky's *What Is To Be Done?* Lavrov's book became the bible of the young radicals and revolutionaries who were shocked by the amorality of Nechaev and were searching for an ethical humanist base for their socialism. These, then, are the two men through whose eyes we view the events of the decade, beginning with the arrest and extradition of Nechaev and ending with the arrests, intrigues, and miseries which the revolutionary movement underwent after the assassination of Alexander II in 1881. Most of the 341 letters, in fact, were written by Lavrov; only 32 of Lopatin's have survived.

The second volume contains a much more varied group of documents, drawn from Lavrov's archives. There is the correspondence of a number of relatively major figures in the Russian movement; there are brochures, pamphlets, statements of po-

litical aims. There are a comparatively small number of letters dealing with Lavrov's journal, *Forward* (*Vpered*), but that was the subject of the first two volumes in the series, also edited by Sapir.

A number of these documents shed further light on important episodes in Russian revolutionary history. Historians will be particularly interested in the substantial body of material on the People's Will in exile, including letters by L. A. Tikhomirov, M. N. Oshanina and the Degaev brothers. There are letters bearing on George Plekhanov's relations with the People's Will émigrés and the eventual founding of the Emancipation of Labor group. From a somewhat later period, Sapir provides us with additional evidence of the anger and demoralization in émigré radical circles occasioned by Tikhomirov's apostasy (1888) and return to Russia. From the same period, we are given several long letters by participants in the abortive conspiracy against Alexander III, describing the attitudes of the young conspirators and giving an interesting, if perhaps idealized, portrait of Lenin's older brother, Alexander, who was hanged on May 8, 1887. Finally, there are some interesting documents on Lavrov's participation in Populist groups in the 1890s, most notably the Group of the Old *Narodovol'tsy* (the latter, untranslatable term meaning member or adherent of the People's Will).

Sapir's edition is a fine one, although his introduction might have concentrated more on the material at hand and less on the generalities of Lavrov's career. His notes are careful and informative. I found comparatively few misprints and typographical errors, although several are particularly glaring, as they pertain to dates (Volume 1, table of contents, page lvi).

ABBOTT GLEASON  
Brown University

S. M. FAL'KOVICH. *Proletariat Rossii i Pol'shi v sovetsnoi revoliutsionnoi bor'be (1907-1912)* [The Proletariat of Russia and Poland in Their Joint Revolutionary Struggle (1907-1912)]. (Akademiiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 377. 1 r. 51 k.

S. M. Fal'kovich, a noted Soviet student of Russo-Polish revolutionary ties, attempts to locate his main concern, Bolshevik-SDKPiL (Social Democracy of the Kingdom of Poland and Lithuania) relations, 1907-11, within the broader context of Russo-Polish relationships. He deals with five major topics: party analyses of the Revolution of 1905 and postrevolutionary tactics; the Third Duma, especially the activities of the nationalist Polish Circle; technical cooperation between Russian and Polish social democrats; mass activity, 1910-11; and interparty relations, 1907-11. The

book is commendable for its vast research endeavor and for its willingness to deal with issues such as Trotsky's efforts to influence the SDKPiL and the left wing of the PPS (Polish Socialist Party) and the role of Rosa Luxemburg much more objectively and openly than have other Soviet scholars.

The author's main thesis is that during this period the "Leninists" and SDKPiL maintained a close alliance that demonstrated the brotherhood of the Russian and Polish masses. Fal'kovich views the period as one of triumph for Russian-Polish social-democratic friendship. His evidence, however, suggests that it was a moment of tragedy for Polish social democracy. Having tied its fate to that of the Russian revolutionary movement, rather than to the Polish proletariat, SDKPiL devoted its energies to maintaining a cohesive and united Russian social-democratic movement. Ideologically, SDKPiL felt itself deeply in agreement with the Bolsheviks, yet from 1906 to 1912 it just as strongly opposed Lenin's efforts to exclude the Mensheviks and other foes of Lenin from the party.

Eventually, the SDKPiL was faced with choosing between loyalty to Bolshevik ideas and party unity. Failure to support the "Leninist" coup at the 1912 Prague conference and unwillingness to accept the ideological position of the Mensheviks and Trotskyists left the Polish social democrats isolated. Once the split became irrevocable, SDKPiL lost much of its power within the party and became a mere appendage of the "Leninists." Thus, this triumph of Polish-Russian friendship had tragic consequences for Polish social democracy.

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St. Cloud State University

DANIEL F. CALHOUN. *The United Front: The TUC and the Russians, 1923-1928*. (Soviet and East European Studies.) New York: Cambridge University Press. 1976. Pp. xi, 450. \$27.50.

Daniel Calhoun examines the tortured relationship between Soviet Russia and the British Trades Union Congress in the years from 1923 to 1928. He finds that in pursuing its goal of a Soviet-dominated international trade union movement, Moscow failed to define the precise aims of this objective. Zinoviev, Lozovsky, and Tomskey each had his own view, and while they argued, Trotsky criticized from the wings. Stalin finally resolved the issue by providing his own focus for the policy and then liquidated it entirely in the final struggle against the "United Opposition." Prior to this the Russians had found willing subjects in Britain on

whom to test their theories of united front tactics. The left wing of the TUC was ready, even eager, to cooperate in an effort to realize international trade union unity. Moscow, however, demanded too much of them. It expected the British Left not only to take positive stands on specific issues but also to act in support of those stands. This proved unpalatable to the British, and when they protested against the vilification which Moscow heaped on them, Anglo-Russian trade union cooperation collapsed.

"The tedious and apparently interminable campaign for international trade union unity," as Calhoun describes his subject, could easily have led to an equally tedious and virtually unreadable book. This is not the case. Calhoun is a fine craftsman and an excellent writer. He succeeds in bringing the reader face to face with all the complexities of trade union politics, united front tactics, and the ferocious struggle for power in Moscow. This confrontation is fruitful, for it serves to highlight the fluidity and multi-tiered nature of the political process involved and illuminates the many influences to which Soviet and British leaders were subject while shaping their policies. It provides an effective antidote to any determinist interpretation seeking to force the exuberantly human experience of this era onto a procrustean bed of assumed "historical necessity."

RICHARD K. DEBO  
Simon Fraser University

JOHN J. STEPHAN. *The Kuril Islands: Russo-Japanese Frontier in the Pacific*. New York: Oxford University Press. 1975. Pp. xiii, 279. \$20.00.

The Kuril Island chain stretches 1200 kilometers to the north of Japan. In recent decades governmental security policies, first Japanese and now Soviet, have combined with impenetrable fogs and difficult and dangerous currents to shield the Kurils from view. The islands provided the marshaling point from which the Japanese fleet sailed to attack Pearl Harbor. Transferred to Soviet control by Roosevelt at Yalta, they now constitute an angry point of controversy between Japan and the Soviet Union, the barrier to a peace treaty and fishing rights desired by the Japanese and to Japanese cooperation in Siberian resource development desired by the Russians.

John Stephan's admirable account supplements his earlier history, *Sakhalin* (1971). As in the other volume, he draws expertly on Russian and on Japanese scholarship to provide rounded coverage of a little-known area, treated previously in English chiefly by George Lensen in *The Russian Push toward Japan* (1959). Chapters take up the geography and prehistory of the islands, rival Russo-



Japanese accounts of discovery and exploration, the islands' importance in border negotiations and frontier issues between Russia and Japan from 1750 to 1875, Japanese rule from then to 1945, the Kurils' role in World War II, their present role as "Soviet outposts in the Pacific," and the continuing Russo-Japanese controversy over the southern part of the chain. At every point the author's account is distinguished by measured objectivity as well as by impeccable scholarship.

Although both Russian and Japanese writers claim responsibility for the "discovery" of the chain, Stephan shows that neither side has a clear case. Japanese probably touched on the arc earlier, but documented (eighteenth-century) contacts are limited to the southern islands. Russians began at the other end at approximately the same time, and other European explorers in the late eighteenth century verified discoveries already made and helped spread word of the area's inhospitable nature. In 1875 the Treaty of St. Petersburg between Japan and Russia united the islands under Japanese rule. Both sides to that agreement considered Sakhalin the more important and desirable of the areas at issue, and Russian success in gaining title to it was accompanied by a willingness to give up the northern Kurils to Japan. During the era of Japanese administration that followed Shikotan and Habomai, the southernmost islands, were never regarded by the Japanese as part of the Kurils, but administered as part of Hokkaido. Stephan's discussion of Japanese rule and his picture of the isolation and loneliness of life for the few who lived there are particularly good.

Japan did not begin to fortify the Kurils until 1940. Stephan deftly summarizes the important role of the islands as Imperial Japan made the decision for war with the United States instead of with the Soviet Union. Also useful is his account of the way, at Yalta, "Roosevelt disposed of the Kurils with breathtaking dispatch." When Stalin opened with, "I only want to have returned to Russia what the Japanese have taken from my country," the president replied with, "That seems like a very reasonable suggestion from our ally. They only want to get back that which has been taken from them." The author's judgment is similarly crisp; "Several good-natured phrases thereupon nullified several treaties and revived a frontier issue that had lain dormant for seventy years. Although the resulting protocol placed the Kurils in a separate category from southern Sakhalin, there is no indication that the chief executive very clearly distinguished the crucial differences between them" (p. 155). As a result, "Neither Russians nor Japanese have forgotten what the war brought to the Kurils. Nearly thirty years after the cessation of hostilities, two countries which fought

for only two weeks and which now share an almost limitless potential for economic cooperation cannot sign a peace treaty because of the Kurils."

Stephan provides a fine account of the "Northern Territories problem" that Japanese politicians talk about. When the Japanese who negotiated the San Francisco Treaty of Peace referred to the Kurils they "meant all of the islands lying between Hokkaido and Kamchatka with the exception of Shikotan and the Habomais" (p. 200). In the negotiations that concluded the state of war with the Soviets in 1956 the Russians agreed to return Shikotan and the Habomais, but they refused even to discuss Kunashir and Iturup. Japan, however, gradually took the position that those two islands were not even part of the Kurils but, instead, "southern Chishima" and thus outside the scope of the San Francisco agreement. With the passage of time positions have hardened on both sides.

This excellent monograph provides essential background for one of the most tenacious territorial problems remaining from World War II.

MARIUS B. JANSEN  
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## NEAR EAST

M. A. SHABAN. *Islamic History: A New Interpretation*. Volume 2, A.D. 750-1055 (A.H. 132-448). New York: Cambridge University Press. 1976. Pp. ix, 221. \$19.50.

The field of Islamic history is jumping out of the frying pan into the fire. Recently, many authors have asserted new methods and standards but have not been able to apply them consistently. This book, like M. A. Shaban's other works (*The Abbasid Revolution and Islamic History: A New Interpretation*, Vol. 1) reflects this malaise. It is an outline of the political, military, and administrative history of the caliphal lands which emphasizes trade and taxation. The author's method involves a careful definition of terms, a suspicion of old interpretations, a new look at sources, and a broader perspective. In each of these, the work displays notable advances but also noticeable shortcomings.

Among the terms and interpretations freshly analyzed are the nature of the early vizierate, the political and administrative significance of Ma'mun's adoption of Mu'tazilism, the effect of mineral production and taxation on Azerbaijan revolts, the relationship between the Saffarids and the Zanj, the meaning of the term "Zanj," the extent of the use of eunuchs, the origin of the Ghaznavid founder Sebuktigin, and the role of trading considerations in the motivations for Fatimid expansion. But often Shaban continues to take



the sources on which his work depends, like Baladhuri, Ibn al-Athir, and especially Tabari, at face value, trusting and repeating their contents where relevant, even though these sources have yet to be subjected to thorough historiographical and stylistic analysis. Furthermore, many key terms are used rather casually, for example, "Islamization," political orthodoxy and Shi'ism. In fact, the spectrum of phenomena associated with the word "Shi'ism" is not adequately differentiated, as indicated by the unclear use of "quasi-Shi'ite" (p. 44) and "pseudo-Shi'ite" (p. 82).

Shaban achieves a broader perspective in important ways: in giving a fuller and more balanced account of the period after Mutawwakil than any other general work and in relating caliphal politics and provincial unrest to trading interest and taxation policies. But he does not create a set of major themes to be followed, nor has he written an analytic conclusion. Neither does he fit this period, whose unity could be questioned, into a long-term context, or the lands of the Abbasids into a wider geographical area, even though he himself suggests continuities. Furthermore, his concentrations on the caliphs' administrative acts and on the central lands, almost to the exclusion of religious, cultural, intellectual, and to a lesser extent institutional history, seems to invalidate the title and produces a simplistic view of motivation. The work seems more a newly-interpreted history of the Abbasid dynasty than a broadly based "Islamic" history. Finally, Shaban evaluates, and frequently criticizes, the governance of the Abbasid empire according to non-comparative values, with sometimes amusing consequences: "These malpractices were so widespread that they were officially accepted as the norm. . . ." (p. 90).

MARILYN R. WALDMAN  
Columbus, Ohio

MARIAN KENT. *Oil and Empire: British Policy and Mesopotamian Oil, 1900-1920*. New York: Barnes and Noble. 1976. Pp. xiii, 273. \$24.50.

For some years historians of the Middle East have carefully avoided the morass of early twentieth-century oleaginous diplomacy, partly because of a plethora of confusing documentation, partly because, until after World War I, it often seemed that petroleum had no more connection with diplomacy than any other commercial product. Marian Kent of the University of New South Wales has attempted to fill this gap in her study of Iraqi oil, with some asides on Persia. With diligent scholarship, she wends her way through the extensive British source material, public and private, outlining every step of the prewar Anglo-German discussions, and the wartime and postwar Anglo-French arrangements for Mesopotamian riches,

ending with the Mesopotamian Agreement of 1920. Very wisely she avoids the obvious pitfall of making her subject the focus of Middle Eastern negotiations when, as she freely admits, it so often was not.

Unfortunately the book carries with it much of the heavy luggage of a thesis. All but the expert will experience heavy going in the details of shares, percentages, and concessions which pass in steady succession. Kent's thrust is sometimes hard to perceive; chapters labeled "analysis of . . ." are more often simple description. The actual text occupies slightly over half the book: notes, a full bibliography, fifty pages of appendixes, even illustrations of some of the principal negotiators are laid on with too heavy a hand.

But there are some interesting lessons. The complex relationship of the several oil companies with each other and with the Foreign Office—above all the still rather inexplicable British hostility to Shell—is a good example of pre-World War I government in the economic sphere. This is the sort of study which is badly needed. The fact that the object was oil in general, rather than Mesopotamian oil in particular, is not the author's fault. In the further studies she promises, however, it will be less easy to separate the issues of a national oil policy from the specific problems of Iraq or Persia in the manner adopted in this volume.

BRITON C. BUSCH  
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YONATHAN SHAPIRO. *The Formative Years of the Israeli Labour Party: The Organization of Power, 1919-1930*. (Sage Studies in Twentieth Century History, volume 4.) Beverly Hills, Calif.: Sage Publications. 1976. Pp. 278. \$13.50.

This book fills an important gap in historical writing available in English on the history of Zionism. The work of a political sociologist, it seeks to explain the rise of labor to spiritual and organizational dominance in Jewish Palestine in the 1920s. In particular, it demonstrates how firm was the grip on power of the leaders of the second *Aliya*—the wave of immigrants from Russia before 1914.

This is a book about mandatory Palestine in which the Arabs and the British hardly appear; they are rightly absent, for the essence of this story is the construction of a self-contained, almost hermetic society, dominated by the labor movement, and its chief organ, the *Histadrut*, the unified Jewish labor union. The fierce ideological conflicts of this period between labor and its opponents, and within the labor movement itself (culminating, for example, in the return to Russia of one disgruntled little group—there to build another New Jerusalem—mostly in the prison camps) should make exciting reading. In fact, however, this book disappoints by dullness.

There are other defects as well. The English is poor; misprints abound; slips in transliteration recur. Moreover, some of the sociological argument is surprisingly shallow. We are told, for instance, that before arriving penniless in Palestine the immigrants of the second *Aliya* had "left comfortable middle-class homes" (p. 12). The only data cited in support of this questionable thesis are figures showing that about seventy percent of the fathers of political activists in Palestine in the 1920s were "merchants and shopkeepers." Of course, all these terms are much too loose; certainly there are grounds for doubting whether the backgrounds of most of these immigrants were "comfortable."

A small factual error should be mentioned: the Zionist Commission in Palestine in 1918 is stated (p. 17) to have been "treated at the time as the provisional government of Palestine." In fact, it was treated as nothing of the sort—as reference to the autobiography of the Commission's chairman, Chaim Weizmann, indicates.

In spite of these serious defects, this monograph will be of use to specialists in the field. The author has, moreover, provided a boon for lexicographers in the shape of several intriguing neologisms; readers of the *AHR* (although not yet of the *OED*) may ponder, for example, on the author's evocation of "historiosophistic images."

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## AFRICA

LADIPO ADAMOLEKUN. *Sékou Touré's Guinea: An Experiment in Nation Building*. (Studies in African History, number 12). London: Methuen; distributed by Barnes and Noble, New York. 1976. Pp. xii, 250. \$17.00.

Ladipo Adamolekun gives us the first contemporary study in English of one of the least known African countries today. His book is the fruit of visits to Guinea in 1968 and 1970–71, talks with local leaders there, and the study of documents some of which are available only in that country. Adamolekun has written mainly about Guinea's political institutions, notably the all-embracing role played by its mass *Parti Démocratique de Guinée*, and to a lesser extent about its economic and social evolution. His material is organized well, his style clear and concise though somewhat repetitious, and his attitude fair and reasonable although he is clearly partial to Sékou Touré.

Theories about nation-building provide the bases for Adamolekun's case study of Guinea and its charismatic leader. High marks are given to Touré as the promoter of Guinea's political independence through flexible pragmatism that he masks behind ideological rigidity. The author con-

siders Touré's failure to develop the country economically and his alienation of the "bourgeois elite" to be regrettable but of minor importance compared with his major accomplishments. Among these are the marked diminution in ethnic rivalries, improvement of the status of women, and the bridging of the generation gap. In short, Adamolekun believes that Touré has succeeded so well in building a nation that his work will resist the alienated bourgeoisie's attempts to undermine it from outside and inside the country, and that his work will survive his incumbency as head of state and party.

The picture of Touré that emerges is that of an astute politician and a sincere African nationalist with Marxist trimmings. Except in the development of mineral resources little is said about Guinea's foreign relations, notably the encouragement Touré has given to subversive activities in neighboring countries whose governments he regards as reactionary or counterrevolutionary. Adamolekun also softpedals Touré's liquidation of his enemies, his persecution complex, and his failure to raise his people's living standards and permit them to enjoy democratic freedoms. Yet this book makes an important contribution because of its factual data and even more because it presents the viewpoint of a highly qualified Nigerian scholar about a prominent fellow African leader.

VIRGINIA THOMPSON  
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NEVILLE CHITTICK. *Kilwa: An Islamic Trading City on the East African Coast*. Volume 1, *History and Archaeology*; volume 2, *Finds*. Nairobi: British Institute in Eastern Africa. 1974. Pp. xviii, 256; xi, 259–514. £25.00.

JAMES KIRKMAN. *Fort Jesus: A Portuguese Fortress on the East African Coast*. London: Oxford University Press. 1974. Pp. xvi, 327. £12.50.

These two works will be required not only in every library concerned with African studies, but also in all those libraries concerned with the history of the Indian Ocean as a whole. Kilwa is by far the largest and most important medieval Islamic town site in eastern Africa, while Fort Jesus was not only the Portuguese headquarters until 1698, but the seat of the Mazar'i governors subject to Oman until 1837. The two sites provide a record of the imports of non-perishable artifacts from the eighth century A.D. until the nineteenth. Thus, taken with other sites in Tanzania, Kenya, Zanzibar, and Somalia excavated by Neville Chittick, and in Kenya and Pemba by James S. Kirkman, we now have a portrait of economic and social life, and of commercial connections stretching over eleven centuries on a long line of coast.

Chittick has cleared (and carefully conserved)

about one-twentieth of the Kilwa town site, including all the known principal buildings. The market, the commercial quarter, the pottery kilns and mint have not yet been found. Extensive bush made aerial photography fruitless, and thus ninety-five percent of the site awaits future research. The finds, local pottery, imported Islamic glazed wares, Chinese and other Far Eastern porcelains, are admirably described. Regrettably, much of the chronology remains obscure, and in particular that of the coins. Only in a very general way can the findings be related to the two histories in Portuguese and in Arabic, not to mention an unreliable Swahili history. The plans and photography are impeccable, and altogether the work constitutes a major contribution to our knowledge of the area. Much more will be intelligible when more becomes known of sites in Yemen, in Southern Yemen, and in the Gulf and western India.

Kirkman's work crowns a quarter-century of devoted work in Kenya, during which he excavated most of the coastal monuments. Fort Jesus of Mombasa, erected in 1593-96 by the Italian engineer Giovanni Batista Cairato with local materials and labor, is the largest Portuguese fort in East Africa other than that of Mozambique. Kirkman describes the plans and architecture of the fort, and the finds of ceramics, glass, beads, cannon, cannon balls, and minor objects in meticulous detail and with exhaustive illustrations. Very few pages are devoted to the history of the fort. Nevertheless, this intensive study is rewarding, not only for the economic record provided by the finds, but because it makes intelligible the topography of the fort and of Mombasa, and so illuminates an account of the Mombasa rising of 1631 now being prepared for publication by me from a unique manuscript in the Augustinian Archives in Rome, and a *History of the Mazar'i*, currently being edited by Bradford Martin, which covers the history of Mombasa from 1734 onwards. It will be invaluable too for the interpretation of the anonymous Portuguese *Historia de Mombaça*, which gives a graphic, detailed account of the siege of the fort by the Omani Arabs in 1696-98, which is in the hands of Kirkman himself. There are a few trifling slips. The Sultan of Mombasa murdered in 1614 was al-Hasan b. Ahmad (not Yusuf). The Portuguese *Vigario Forane* is not an "external vicar," but the Latin *vicarius foraneus*, ordinarily rendered "rural dean." The printers have muddled the order of the index: H to Z is printed after plate 41 and immediately precedes plate 44; A to He follows, and then plate 42 and plate 43.

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HARALD R. BILGER. *Südafrika in Geschichte und Gegenwart*. Konstanz: Druckerei und Verlagsanstalt Konstanz. 1976. Pp. 808. DM 58.

Harald R. Bilger is an engineer and economist by profession, not a professional historian. Having worked in Africa for many years, he has turned to writing history. The present volume is the most detailed modern study of South Africa's evolution that exists in German. He begins with a survey of southern Africa before the arrival of the whites. He ends with a discussion of South Africa's most recent problems, written from the standpoint of a Christian who rejects South Africa's racial policy on religious, ethical, and social grounds.

Like all general works, this book necessarily has its weaknesses. The bibliography is overloaded with ephemeral newspaper articles. Specialist studies such as S. D. Neumark's *Economic Influences on the South African Frontier* (1956), or P. J. van der Merwe's *Trek, studies oor die mobiliteit van die pioniersbevolking aan Kaap* (1945) have not, apparently, been used. Neither do such general histories as A. J. H. van der Walt, J. A. Wiid and A. L. Geyer, *Geskiedenis van Suid Afrika* (1951, 2 vols.), C. W. de Kiewiet, *History of South Africa: Social and Economic* (1941), or the South African volume of the *Cambridge History of the British Empire* (vol. 8, 2d ed., 1963) appear in the bibliography.

Specialists will take issue with many of the author's interpretations. For instance, John Mackenzie is accused of having abused his missionary office for imperial purposes in Botswana. Mackenzie, in fact, used his political office for the purpose of defending the Tswana against the Boers and the British South Africa Company. Like most of his evangelical contemporaries, he regarded British imperial protection as a safeguard for the people whom he meant to serve. The author is likewise mistaken in the assertion that the Communist Party of South Africa, having been Stalinized, turned into a black party; the effective positions of leadership at the time remained with South Africans of British and Jewish descent. Many statisticians will dispute his statement that blacks today cannot rise from the ranks of the lowliest paid workers. Africans are certainly remunerated in a more adequate fashion than they were, say, thirty years ago; there has also been a striking change within the ethnic composition of the South African labor force that now contains far more skilled Africans than at any time in previous history. There are also gaps. The rise of Afrikaans literature, for example, might have at least merited a page or two; so would the role of Jewish South Africans, an important minority within a minority.

Nevertheless, Bilger's book is a good sample of *haute vulgarisation*. The book is well organized. It is massive. There are maps, photographs, and statistical tables aplenty. The author does endeavor to cover the entire range of South Africa's problems, from early culture contacts to white immigration (including what he calls "The German Peril"), and modern industrialization. He is familiar with

problems of economic entrepreneurship, Calvinist theology, naval strategy, and black consciousness. His historical interpretations often happen to conflict with those of the present reviewer, but they are always suffused by common sense. What critic could ask for more?

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## ASIA AND THE EAST

VITALY A. RUBIN. *Individual and State in Ancient China: Essays on Four Chinese Philosophers*. Translated by STEVEN I. LEVINE. New York: Columbia University Press. 1976. Pp. xxix, 149. \$9.95.

This work presents a brief and highly readable introduction to four of the principal philosophical schools of the Warring States period—Confucianism, Mohism, Legalism, and Taoism—through an examination of the lives and major works of Confucius, Mo-tzu, Shang Yang, and Chuang-tzu. Vitaly A. Rubin skillfully outlines what little we know of their biographies, and sketches their positions on various social and ethical issues against a background of political events. Some attention is paid to the later development and influence of each of the four schools, though that is done only briefly. The book could be used either to supplement or to replace Waley's standard, but flawed, *Three Ways of Thought in Ancient China*; the comparison indicates how highly this work must be regarded as an introductory text.

Nor is the value of the book limited to introductory teaching. Specialists in the history of Chinese thought will applaud Rubin's treatment of the contrasting principles of *wen* (culture) and *wu* (military prowess) in the several schools, and his interesting and instructive derivation of many of the principles of Legalism from the Mohist school. Moreover, Western Sinologists are accustomed to viewing ancient Chinese thought against their own background of traditional liberalism; reading the work of a Russian colleague who has had direct experience with a statist, totalitarian regime forces us to take another look at the principles of the ancient schools.

A specialist will also be inclined to take issue with some points. Thus, in stressing the connections between Mohism and Legalism, Rubin slights the relationship between Legalism and the Lao-tzu school of Taoism (for example, Lao-tzu 19, "Exterminate the sage, discard the wise, and the people will benefit a hundredfold"). By the same token, one wishes that the differences between the Legalist-tinged Taoism of Lao-tzu and the elitist anarchism of Chuang-tzu had been more clearly explicated. Perhaps because when the Rus-

sian edition of this book was published (1970) the main body of H. G. Creel's work would not have been available to him, Rubin dismisses too quickly Creel's discovery of the work and influence of Shen Pu-hai. Ideally, any treatment of Shang Yang and his successors would give more weight to the now generally accepted theory that the *fa-chia* really consisted of two separate though related schools of Legalists and Administrators.

These faults, however, detract but little from a good and useful book. Included in an appendix is a complete bibliography of the author's works, which will be welcomed by specialists inasmuch as Rubin's writings have been suppressed in the Soviet Union, and even the titles are hard to find in Russian bibliographies.

The publication of the English edition of this book coincides with Vitaly Rubin's release from the Soviet Union after several years of harassment as a "Jewish refusednik." The world of Western scholarship will be pleased to welcome Russia's most distinguished Sinologist into its ranks.

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JOSEPH NEEDHAM, with the collaboration of HO PING-YÜ and LU GWEI-DJEN. *Science and Civilisation in China*. Volume 5, *Chemistry and Chemical Technology*; Part 3, *Spagyric Discovery and Invention: Historical Survey, from Cinnabar Elixirs to Synthetic Insulin*. New York: Cambridge University Press. 1976. Pp. xxxv, 481. \$42.00.

With this history of alchemy, Joseph Needham and his collaborators again supply new profound data, interpretations, and questions for understanding the ancient Chinese world. That there was a continuous and uniquely Chinese motivation driving the alchemical arts for at least 2000 years is a major contention of this treatise. This motivation, born in the spirit of Taoism, is the belief that health-immortality can and should be sought by both the making and taking of elixirs such as artificial gold, representing the imperishable product of mineral transformations, as immortality is the ultimate of bodily functions. By a detailed analysis of the literary works by and about the alchemists the authors can literally construct a continuous master-disciple filiation for more than eighty participants active from the second to the fourteenth centuries. Especially valuable is the thorough treatment of Wei Po-Yang's *T'han Tung Ch'i* and the Taoist repository, the *Tao Tsang*, which apparently served as standard guides through the ages. This scholarship reveals convincing evidence that, in spite of the antiorthodox and esoteric nature of alchemy, there was an active tradition rising through a Golden Age (400 to 800) before gradually diminishing.

Germane to the development of this tradition



and the uniqueness of Chinese alchemy is the author's abundant demonstration that the early merger of external (laboratory-matured elixir) alchemy with internal (physiologically perfected elixir) alchemy produced the Golden Age. Likewise, alchemy subsided from the ninth century, a development corresponding with a decrease in external alchemy. The explanatory evidence given to explain this decrease is the frustration with lack of success and the growing skepticism of the claims of the ancient "sooty empiricks." It is noteworthy that although the disdain for alchemy by Confucianists and Buddhists is offered as a sociopolitical factor, the above "internalist" arguments for the fall (and the rise) of alchemy are significantly more documented in this treatise.

Along the way this book includes tidbits valuable to anyone seriously interested in China, including alchemy's relationship with T'ang poetry, Ming novels, the first printed science book, imperial laboratories, ladies, Japan, the Jesuits, conceptual symbols, a possible conception of oxygen, and the problems of transliteration into modern chemistry terminology. My concern is that the partition of alchemy into four books has created redundancy, including the significant bibliography, and weakened the author's purpose. Surely apparatus and theory are important to history, and the omission of data on internal alchemy leaves a gap in the story told here. Nevertheless, this book is essential as it has established the integrating theme for Chinese alchemy and offered a demonstration of a "normal" Chinese science, both of which must be considered in future statements about Chinese thought.

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Bloomington

MORRIS ROSSABI. *China and Inner Asia: From 1368 to the Present Day*. New York: Pica Press; distributed by Universe Books, New York. 1975. Pp. 320. \$20.00.

Inner Asia, the region of Mongolia, Manchuria, Soviet-Chinese Turkestan, and Tibet, has been a critical area in the history of Imperial China. It has become more so since the Sino-Soviet conflict has given these border areas a new strategic, economic, and political importance.

A number of studies have dealt with the problems of China's relationship to Inner Asia, most notably Owen Lattimore's *Inner Asian Frontiers of China*, in which the author formulates some challenging theses on the relationship between Chinese agricultural society and the nomad societies of the steppes, the Tibetan highlands, and the forest tribes of Manchuria. Morris Rossabi seeks to de-

scribe again the Chinese relationship to Inner Asia in imperial times and to compare it with the recent problems of the Chinese Communists. As the title implies, the author looks at Inner Asia from China.

The book is directed to the general reader and is indeed a useful, quite detailed history of the Chinese-Inner Asian relationship under the Ming and Ch'ing dynasties and finally under the Chinese Communists, making full use of all the many important secondary works. It is a good and useful account, well written, and a survey that will serve well as an introduction to the area and its history.

As always, one can argue with the author on a number of points. It is regrettable for instance that he omitted to deal with Tibet, and his argument that Tibet did not pose a military threat during later imperial time and has played no role in the Sino-Russian border conflicts does not eliminate Tibet as the most startling example in our time of a Chinese Communist policy of genocide, particularly since Rossabi makes an attempt to assess Chinese Communist policy toward the minority peoples. Here one should point out that in his comparison and analogy between Chinese imperial and Communist policies toward the region, the author may have underplayed the difference between Confucian imperial policy that did not aim at destroying the culture of the peoples of the area, and the Communists' present attempt aiming not only at "sinicizing" the non-Chinese groups, as the author states, but also at communizing them. One may argue whether the Chinese Communists have promoted "a better life for the average herdsman, farmer or town-dweller in Inner Asia" as they proclaim. Furthermore, special characteristics of Chinese Communist policy could perhaps have been presented more strongly, to underline the point that, in spite of all similarities, Peking's policy toward Inner Asia belongs to a different world than that of the imperial past. But if the interpretation could have been strengthened, the story is well presented.

FRANZ MICHAEL  
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ERIC WIDMER. *The Russian Ecclesiastical Mission in Peking during the Eighteenth Century*. (Harvard East Asian Monographs, number 69.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press. 1976. Pp. xi, 262. \$15.00.

In the late seventeenth century, a number of Russians from the remote Siberian garrison of Albazin strayed into China and were captured. They were detained in Peking, and given food, housing, wives, and a Buddhist prayer house in which to practice their religion. When the only Russian Orthodox



priest among the captives died, permission was granted for several Russian priests to join them, and the Russian ecclesiastical mission in Peking was thereby established.

There were seven official mission delegations between 1716 and 1794, at approximately ten-year intervals. The Treaty of Kiakhta (1728) provided for an Orthodox church to be built in the Russian hostel in Peking, where up to four priests and six language students could live. Long before the Western European powers, then, Russia had "representatives" in the Chinese capital.

Eric Widmer's excellent monograph on this long-neglected subject contains a detailed narrative of the origins and early years of the mission (1683-1729) and a briefer narrative of subsequent missions through 1794. He also discusses the major institutions connected with the mission, and assesses it in terms of missionary activity, Russian Sinology, and eighteenth-century Sino-Russian relations.

The problems of the mission were legion: countless delays, misunderstandings, frustrations, hardships, reluctant students, and missionaries afflicted with homesickness, demoralization, drunkenness, and disease. Far from converting the Chinese to Christianity, the mission frequently failed even to prevent the "heathenization" of the Albazinians.

Despite all this, as Widmer effectively argues, the mission was significant in serving the differing purposes of the Russian and Chinese empires. To the Russians it offered the pretense of an "embassy" and an "ecclesiastical foothold" (to rival and even outlast the Jesuits) in Peking. To the Chinese it symbolized the paternalistic generosity of the Manchu emperors toward "Inner Asian barbarians" and the cosmopolitan importance of Peking for all "Inner Asians." Because of the very obscurity of the mission, these divergent views co-existed without causing serious problems. Widmer demonstrates that the mission did contribute significantly to the beginning of Russian Sinology and to the stability of Sino-Russian relations—a stability that facilitated the mid-eighteenth-century expansion of Chinese control over the steppe peoples to the west.

Heavily based on Russian sources (supplemented with a variety of Chinese- and Western-language materials), this study is particularly valuable for the Sinologist in illuminating Manchu-Chinese foreign policy from a new perspective. The book could be improved with a Chinese glossary and a brief bibliographical essay, but these are minor shortcomings in a fascinating and useful work.

PAUL S. ROPP  
Memphis State University

FRED W. DRAKE. *China Charts the World: Hsü Chi-Yü and His Geography of 1848*. (Harvard East Asian Monographs, number 64.) Cambridge, Mass.: East Asian Research Center, Harvard University, 1975. Pp. ix, 272. \$8.50.

This book, part biography and part translation, introduces Western readers to one of the two classic world geographies written in China in the 1840s. Both were products of the concern of officials on China's southeastern coast, who initially confronted the overwhelming superiority of Western arms in the Opium War of 1839-42. These works were the first to attempt to place China in the global perspective of European expansion, and the writers concentrated on obtaining facts about peoples hitherto dismissed in stereotyped terms as barbarians. Hsü Chi-yü's book, which became a standard reference work in the libraries of China's early advocates of modernization, was one of the first to speak of "self-strengthening" programs. It was unique in its time for the author's reliance on Western sources, and for his consultation with Westerners of all stripes who resided in China.

Fred W. Drake's book includes reproductions of selected maps from Hsü's work and extensive translated excerpts, so that it can be used as a brief reference work to summarize views of the non-Chinese world that were enlightened in their day. Hsü described individual nation-states, gave admiring accounts of representative political systems, and also included a significant amount of diplomatic and strategic analysis of interstate relations, particularly the Anglo-Russian rivalry and the colonial expansion of the Western powers into Chinese spheres of influence.

Unfortunately this study leaves unresolved the question of the place of Hsü's work in China's intellectual history. Since China itself was exempted from an integral treatment in Hsü's geography (p. 68), his warning messages about the European threat were not "hammered home" (p. 98) but merely implied through innuendo and analogy, as in the account of India's colonization (p. 103). Hsü's important description of the commercial tax base of the Western trading powers (p. 116) makes no judgment about the desirability of such commercial orientation, and in fact presents agriculture and commerce as two mutually exclusive options in state finance.

Foreigners were a delicate subject in Hsü's day. Even a work as cautious as the *Ying-huan chih-lieh* was politically charged, and Hsü's own political fortunes were clearly tied to his view of the West, or to his association with a group identified as conciliators. Some broader-based effort to link Hsü's own intellectual heritage (textual research and statecraft) with the political machinations at court and in the provinces would have helped to

integrate biography and intellectual history in this study and would have made it useful to a larger audience.

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BRITTEN DEAN. *China and Great Britain: The Diplomacy of Commercial Relations, 1860-1864*. (Harvard East Asian Monographs, number 50.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press. 1974. Pp. 223. \$6.00.

STEPHEN CHAPMAN LOCKWOOD. *Augustine Heard and Company, 1858-1862: American Merchants in China*. (Harvard East Asian Monographs, number 37.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press. 1971. Pp. xii, 157. \$4.50.

China's international involvement today is extensive, well informed, and designed in China's national interest. It also incorporates significant lingerings from Chinese commercial practices of the past: the Canton Trade Fair, for example, prolongs the concentration-of-trade principle inherent in the treaty-port system of a century ago; presidential visits are reminiscent of tribute missions to the Chinese court. In the long history of Sino-foreign relations, perhaps the most critical period is that which saw adjustments to the unequal treaties initiated by Western nations. The nineteenth-century treaty experience brought both a crystallization of Western attitudes toward China and the stabilization of Chinese proclivities at commercial and diplomatic interaction.

Here are two books examining the China trade around 1860. One pursues Sino-American relations through the Heard family company in its last years of operation on the China coast; the other explores Anglo-Chinese trade through the commercial diplomacy of Frederick Bruce. Both books concentrate on the implementation of arrangements advantageous to Western merchants as specified by the treaties of Tientsin (1858) and Peking (1860). Both elucidate characteristic Anglo-American concern for profit and Chinese efforts to retain as much control as possible in China's foreign relations, despite military weakness verified by two wars with Great Britain. Both books also suggest the limitations of Anglo-American commitment to the uncertain China market, regardless of profit potential.

Britten Dean's monograph is well researched and highly informative. Drawing upon Parliamentary Papers as well as Tsungli-yamen and other documents of China's foreign affairs, Dean has explored in depth the issues of conflict in treaty negotiations involving merchant rights and privileges. At the same time, his sympathetic treat-

ment of both Frederick Bruce and Prince Kung suggests similarities between the two men and illuminates their common commitment to cooperation in the aftermath of the Anglo-Chinese war of 1856-60. In Dean's portrayal, these two men figure prominently in the transition from tribute to treaty in China's external commercial affairs and hence contribute to China's changing order.

Important to Dean's presentation is the clash between Frederick Bruce's vision of a strong central Chinese government and the reality of local power in late Ch'ing China. Prince Kung and the imperial bureaucracy apparently could deal with Bruce's requests for protection of British merchants from trade restrictions and local fees while simultaneously encouraging the continuation of local advantage in revenue generation (pp. 80-84). The complexity of the situation, while well explored with regard to the bean trade, could use more emphasis and analysis in terms of Ch'ing political and economic control generally. There seems more of a story here than Dean's "nuts and bolts" approach (p. 2) and generally narrative account allows.

Stephen Lockwood's study does not offer the factual breadth or historical depth of Dean's work, yet it may be the more engaging of the two books. It began as a senior honors thesis at Harvard (and as such should be an inspiration for undergraduates with similar access to archival materials in English). Spotlighting Heard and Company to the near exclusion of other houses altogether, this admittedly preliminary work reveals the character and competence of some American merchant-agents who met competition in the China treaty ports on behalf of profit-minded constituents at home. Economic conditions in the United States during and after the Civil War ultimately undermined the old commission house structure epitomized by Heard and Company, which reached its peak in 1858-62 and faded thereafter.

The Heard-style agency house became less important in the China trade as economic and technological developments rendered it obsolete. Unlike Russell and Company, Heard's did not adjust to steamship transport and other innovations that brought customers at home more directly into the China market. The Heard family chose instead in the 1860s to withdraw from China trade profit-hunting. The "commercial conservatism" that was both cause and product of Heard's early success also dictated Heard's incompatibility with an expanding China trade (pp. 112, 117-18).

These two books reveal complexities of Western approaches to China and Chinese perceptions of Western nations in an important era of change. They demonstrate the validity and the potential of archival documentary research. They also perpetuate an historiographical preoccupation with the

1860s. Perhaps what scholars need now is a look at the 1840s and 1850s, thus to destroy the myth that in those years there was a consistency which, when contrasted with the diversification of the 1860s, makes that decade look more dramatic and full of change than it really was.

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LO HUI-MIN, editor. *The Correspondence of G. E. Morrison*. Volume 1, 1895-1912. New York: Cambridge University Press. 1976. Pp. xiv, 848. \$62.50.

George Morrison, an Australian with a medical degree from Edinburgh, was the most famous China-watcher of his time. He also expended his considerable talent and energy upon lost causes. As Peking correspondent for *The Times* of London from 1897 to 1912 he tried to guard the interests of British imperialism against the claims of rival powers, while in the background the Chinese nationalist movement was gathering momentum. Then he became the political adviser of Yüan Shih-k'ai, when Yüan, whom Morrison had long promoted as China's indispensable "strong man," became President of the Republic. Yüan pushed the country along the road to warlordism, and after his death in 1916, and until he himself died in 1920, Morrison served successive warlord governments in a minor capacity—a public relations agent who occasionally helped draft correspondence in English.

This volume of selected correspondence, which Lo Hui-min has edited superbly, deals with the first and most important chapter in Morrison's career. He came to Peking in time to cover the Battle of the Concessions. The Boxer Uprising and a premature obituary in *The Times* made him famous and enabled him to participate in the great game of power politics, as well as to observe it. As a reporter he always strived for accuracy and objectivity—he never accepted favors, gifts, or decorations from the officials and governments who were only too eager to win his approval—but his perspective was always limited by his perceptions of British imperial interests. Thus when Germany went into Shantung and precipitated the scramble for concessions, Morrison was satisfied: "If China is to be divided into such spheres it cannot but be our gain to have a German sphere wedged in between us and the Russians." As for the Boxers, "We have lost nothing by the 'outbreak.' As an actual fact we gained greatly in prestige. . . ." He welcomed China's anti-American boycott in 1905 because it "gave stimulus to the trade of the British Empire. . . ." And of course his greatest moment came when Japan defeated Russia in what was sometimes called "Morrison's war." But by 1906—only a year later—he was embarrassing *The*

*Times*, the Foreign Office, and the Japanese government with an anti-Japanese crusade that he was to carry on for the rest of his life.

In denouncing Japanese designs upon China, Morrison was both more perceptive and less realistic than his opponents in London. While they doubted his reports of a developing "national spirit" in China after 1905, they saw more clearly the inevitable contradictions between Chinese nationalism and the British imperial interests which he prized so highly.

Morrison's correspondence, which should be invaluable to students of East Asian diplomatic history, also reveals some unpleasant sidelights of his personality. He seems to have had an unhealthy interest in sexual gossip. He was also an unrelenting anti-Semite.

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DON C. PRICE. *Russia and the Roots of the Chinese Revolution, 1896-1911*. (Harvard East Asian Series, number 79.) Cambridge, Mass.: Harvard University Press. 1974. Pp. 303.

Several intriguing questions have probably crossed the minds of all who have closely studied the Chinese Revolution of 1911: What were the foreign influences on the revolution, and to what degree did the articulate elite of China respond to those influences? And what were the qualitative effects of a particular country, for example Russia, on reformers and revolutionaries of the time?

Sifting through an impressive number of Chinese sources from the 1896-1911 period to analyze the images of and attitudes toward Russia held by a certain segment of China's educated class, Don C. Price not only provides thoughtful answers to the above questions, but further acquaints scholars with a number of writers and writings that have remained obscure or unknown up to now. Among the many topics he discusses, the following might be of special interest: 1.) Chinese perceptions of the world at the end of the nineteenth century; 2.) positive and negative influences of Peter the Great and the Petrine model; 3.) Russian concepts of revolution and their effect on Chinese intellectuals; 4.) Russian imperialism and the response of the intellectuals. A fifth theme weaves itself through the whole book: the author asks whether, "in stressing the extent to which the Chinese revolution embodied genuine Chinese purposes," scholars have "underestimated the importance both of the Russian influence and of a certain indigenous Chinese responsiveness to the appeal of world revolution" (p. 2).

Price reemphasizes a point that scholars too often forget as they try to impose their own ex-

pectations on late Ch'ing China: even at the end of the nineteenth century, "most educated Chinese remained disdainful and abysmally ignorant of the outside world" (p. 12). Though recognizing that K'ang Yu-wei by the end of the century had "squarely faced the existence of a non-sinocentric world" (p. 17), Price gives short shrift to Sun Yat-sen; Sun seems as much ostracized for his Western knowledge by modern scholars as he was by the Chinese intellectuals of the time.

The interplay of reform and revolution and Sino-centrism and universalism were quite evident in Chinese views toward Russia, in particular toward Peter the Great. Beginning with a fascinating, if error-filled, book by Ho Ch'iu-t'ao that discusses Russia and Peter, then continuing on with the arguments of Yen Fu, K'ang Yu-wei, Liang Ch'ich'ao, Wang Shu-nan, and others with respect to the strengths and weaknesses of the Petrine model, Price unfolds an important chapter of China's reform-revolution period. He could have been more emphatic, however, when writing that the Petrine image suffered in China because of Russian expansionism. Not that the author ignores the important topic of Russian imperialism, but the problem is not integrated fully enough into the other chapters of the book. The reaction of both the more articulate elite and the less articulate masses to the emotional questions of Russia's aggression and encroachment in China should be more carefully analyzed.

We could ask Price to follow this informative work with more studies about China's response to the resurgence of Russian populism in the 1880s and 90s; he might wish to study more fully a topic that Benjamin I. Schwartz raised some years ago, namely parallels and influences of the Russian intelligentsia and Chinese intellectuals; and I would like to see Price discuss "Russia and the roots of Chinese socialism" in more detail than he did in this book.

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DWIGHT H. PERKINS, editor. *China's Modern Economy in Historical Perspective*. Sponsored by the Social Science Research Council. Stanford: Stanford University Press. 1975. Pp. xiv, 344. \$13.85.

The last hundred-odd years are a crucial period for China's transition from a traditional to a modern economy. Comprehensive studies of this period have long been considered necessary, but have also been in short supply. This volume edited by Dwight H. Perkins is intended to meet this demand. The papers in it were prepared originally for a conference in Bermuda in June 1973, and give not only a comprehensive survey of China's modern economy, but also an excellent set of references

constituting an important contribution to our understanding of the transition in China's economy.

Perkins' two articles are a great contribution to this volume. "Introduction: The Persistence of the Past" gives a remarkable summary of economic development during the period under study, and "Growth and Changing Structure of China's Twentieth-Century Economy" is a painstaking work dealing with the structural change of China's economy during the period from the 1930s through the 1970s and based on disputed data. Robert F. Dernberger's "The Role of the Foreigner in China's Economic Development, 1840-1949" examines the economic activities of the foreigner in China in the hundred-odd years prior to 1949 and finds that, in spite of some negative features, their activities have made a positive contribution to China's economic development. Carl Riskin's "Surplus and Stagnation in Modern China" indicates that technological considerations come to the fore to explain China's continuing economic backwardness in the modern, pre-Communist era. Mark Elvin's "Skills and Resources in Late Traditional China" points out that technological change in late traditional China was a stabilizing factor in helping to keep per-capita income from falling as population grew and pressure on resources became sharper. Thomas G. Rawski's "The Growth of Producer Industries, 1900-1971" indicates that China's experience does not support the view that massive investment and hard work alone can quickly create a viable sector of producer goods in a backward economy. Kang Chao's "The Growth of a Modern Cotton Textile Industry and the Competition with Handicrafts" gives an excellent analysis of the development of a modern cotton textile industry and a reasonable explanation of the decline of the family production system characterized by the handicraft weaving industry. John C. H. Fei's "The 'Standard Market' of Traditional China" is a theoretical analysis of the "Standard Market," taking transport cost as one major determinant of the shape of such a "market." Ramon H. Myers' "Cooperation in Traditional Agriculture and Its Implications for Team Farming in the People's Republic of China" gives a detailed description of cooperative techniques in traditional agriculture in China. Finally, Peter Schran's "On the Yen'an Origins of Current Economic Policies" gives a plausible argument that the current economic policies of the Communist government are a continuation and development of the Yen'an spirit.

Though some of the papers are original and stimulating, many of the points raised are open to debate. The assertion that China's post-1949 economy was a product of the nation's own past, that the foreigner in China had made a positive contribution to China's economic development prior to



1949, that stagnation in China was mainly due to technological backwardness, and that the Communists' team farming system originated in the cooperation of traditional family farming may be cited as examples. Nevertheless, this volume is a worthwhile contribution to understanding China's modern economy.

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JAMES E. SHERIDAN. *China in Disintegration: The Republican Era in Chinese History, 1912-1949*. (The Transformation of Modern China Series.) New York: Free Press. 1975. Pp. xii, 338. \$12.95.

James E. Sheridan stresses the historical phenomenon of national disintegration in Republican China. While he is critical of Sun Yat-sen and Chiang Kai-shek, he lauds the Chinese Communists for their ability to reintegrate the nation under Mao Tse-tung's leadership. Probably because of his own research interest in Feng Yu-hsiang, Sheridan devotes one quarter of this volume to the study of warlordism. To justify his seemingly excessive attention to the subject, he describes the warlord period as exemplifying "the extremity of China's territorial disintegration" (p. 39).

The book is evidently not a product of original research. The author freely admits to having summarized the writings of other scholars in his text. In the notes, he fails to mention any of the important Japanese publications on the period; only five Chinese works are quoted. Despite these limitations, however, it would be grossly unfair to dismiss *China in Disintegration* as another ordinary textbook for university undergraduates.

In his selection of national integration as the unifying theme for his book, Sheridan demonstrates his grasp of the dynamism of twentieth-century Chinese history. His narrative is an effective combination of his own insight with the recent scholarship of, for example, Odoric Wou, Lloyd Eastman, and Diana Lary. He further benefits from his access to the unpublished materials of the Chinese Oral History Project of Columbia University's East Asian Institute. The result is a refreshing historical account of Republican China, interspersed with daring observations.

These obvious strengths notwithstanding, the book suffers from inherent weaknesses. It is difficult to try to analyze the complexities of the Chinese Republic in less than 300 pages, and Sheridan does not render his task easier by concentrating on warlordism. Consequently, he oversimplifies much of his description of other developments. His discussion of Chiang Kai-shek's role in the Kuomintang of the 1920s is an example. Similarly, he

quotes Ernest Young's study of Yuan Shih-k'ai's rise to the presidency without recounting the details, thus weakening Young's forceful argument.

In his analysis of national integration, Sheridan emphasizes the need for mass participation in politics. He attacks the elitist leadership of both the T'ung-meng-hui and the Kuomintang. Such criticisms are based on a value system totally incongruous with the bourgeois-democratic nature of the pre-Communist revolution in China. They also cast doubt on the validity of the author's general thesis.

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PATRICIA E. GRIFFIN. *The Chinese Communist Treatment of Counterrevolutionaries: 1924-1949*. (Studies in East Asian Law, Harvard University.) Princeton: Princeton University Press. Pp. xi, 257. \$15.00.

This important book examines the turbulent history of the Communist movement in China from a refreshingly new perspective: the development of the legal system as reflected in Communist treatment of counterrevolutionaries from 1924 to 1949. The author divides this span into four periods (the beginning of the Kiangsi Soviet, the crisis of 1933-34, the Yen'an period, and the civil war) posing a series of questions about each stage. What was the environment of the period; how was the legal system organized; who were the counterrevolutionaries; what was the official policy on laws and punishments; and what was the actual practice?

Not surprisingly, environment was the most critical factor in determining treatment of counterrevolutionaries. The era was one of constant struggle, and survival was the pre-eminent concern. Legal decisions were crisis-oriented, not ideal. By necessity the system was decentralized, not formalized. Nonetheless, changes in ideology and leadership significantly influenced legal policy. The definition of "counterrevolutionary" varied. In the Kiangsi period, the term incorporated class enemies based on economic as well as behavioral factors. Because the party leadership was divided, suppression of counterrevolutionaries was used as a cover for liquidation of political enemies. Punishments were generally swift and harsh.

During the Yen'an period, the term "counterrevolutionary" was narrowed to include only those who actively opposed the anti-Japanese war effort. Many of the harsh practices of the Kiangsi period were curbed, and the concepts of leniency, reform, rehabilitation, and flexibility of punishments were stressed. Those who voluntarily surrendered, confessed, and repented were dealt with less severely.



The relative stability and unified leadership of the Yen-an period led to greater regularity in the operation of the legal system.

During the civil war the definition of "counter-revolutionary" was enlarged once again to include class enemies. The Communist leadership had learned from the Kiangsi experience, Patricia E. Griffin concludes, and retained the idea of leniency and the emphasis on rehabilitation adopted during the Yen-an period. The system remained flexible. Favored economic classes, workers and peasants, could receive preferable treatment, but in practice this was only done when convenient.

Griffin concludes that the fundamental role of law was to serve the needs of the state and the Communist movement, not to protect the individual. The development of a legal mechanism to deal with counterrevolutionaries had the utilitarian goal of suppressing very real enemies of the Communist movement. The public trial system was used to educate both the masses and party cadres and to increase popular participation in the movement.

The legal system of the People's Republic retains many characteristics developed in the pre-1949 period: flexibility, a loose structure in comparison to Western systems, and general rather than specific laws. Griffin suggests that traditional Chinese distrust of the formal legal structure and the needs of mass-line politics are the key causes of this development.

The author includes a separate chapter on prison management. Given the exigencies of the wartime economy and the limited security of Communist positions, prison conditions remained harsh despite reform efforts. In the Yen-an period, parole and reform through labor increasingly replaced long-term incarceration.

Sources for the study include the Ch'en Ch'eng collection of documents for the Kiangsi period and the *Chieh-fang jih-pao* and other newspapers and journals for the later periods. Appendices include translations of key legal documents.

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WILMA FAIRBANK. *America's Cultural Experiment in China, 1942-1949*. (Cultural Relations Programs of the U.S. Department of State, Historical Studies, number 1.) Washington: Bureau of Educational and Cultural Affairs, U.S. Department of State. 1976. Pp. xvii, 233. \$5.10.

The present volume is part of a series of books on the history of the international educational and cultural exchange program of the Department of State. Wilma Fairbank surely was well-equipped to survey the eight-year effort (1942-49) to provide

assistance to the Chinese people in such areas as education, public health, and agriculture. She participated in the program during its first six years, and, between 1945 and 1947, was cultural attaché in Chungking and Nanking.

The program of cultural exchange with China began with high hopes, and there was a considerable achievement. It was modeled on an earlier program in Latin America, but the State Department soon discovered that techniques of cultural cooperation in one society did not necessarily apply to another. In Latin America cultural relations were mainly for an elite. In China the program became broader, including both Chinese academics and people from the masses of the populace. In subject, too, there was an effort to reach farther; the Chinese program included science and technology. A pattern evolved through trial and error. Microfilms of important journals in fields such as science, history, and architecture were sent to China, along with microfilm readers. American experts on these subjects went to China. Chinese came to the United States to lecture and observe. An attempt was made to include Communists, but their applications for passports were refused by the Kuomintang government.

The survey of programs was perhaps the least interesting part of Fairbank's study. The story comes alive when the author writes of obstacles to the program. Because of unremitting warfare in China cultural exchanges were sandwiched between crises, internal and external. Inflation often caused problems. The Chinese government fixed a rate of exchange unfavorable to the United States dollar. It asked for experts in specific fields, and the Americans did not question the choice of fields. The United States thus fell prey to manipulation by the Chinese. Fascinating is the description of the manner in which the Kuomintang forced Chinese on their way to America to undergo "thought control." The effort brought a hostile reaction from the American government, and the Chinese quietly dropped it at the end of 1944.

Institutional history is difficult to write. Fairbank's task is to survey an eight-year period in the social, political, and psychological climate of America and China. One has the feeling that the author was forced to say too little about too much. She devotes a fourth of her book to surveying the results of the Fulbright educational exchange, and measures its effectiveness by books and articles. The number is impressive. But is this not trying to measure the immeasurable—the effect of the cultural exchange on China and on America? What effect did it really have? One must hope that whatever the precise result, its achievements were lasting.

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JAY LEYDA. *Dianying: An Account of Films and the Film Audience in China*. Cambridge, Mass.: MIT Press. 1972. Pp. xvii, 515. \$12.50.

More than a decade ago, Jay Leyda—a literary wanderer who had produced books on Emily Dickinson, Herman Melville, Sergei Eisenstein, and Soviet cinema—turned his talents to China, where he traveled and worked from 1959 through 1964. This unusual book interweaves personal diary, history, a series of brief biographies, and a cinematic catalog; its style is personal, empirical, and non-ideological. Since the author neither reads nor speaks Chinese, his viewpoint and values necessarily are Western. But just because he is not Chinese and not a defender of any particular nation or political system, his report attains a certain objectivity.

Leyda's fascinating and provocative survey points to a truth that historians are likely to forget: that the film—no less than literature and art—is a cultural form reflecting and projecting the tensions, fears, aspirations, and transformations of the modern era. And the film, moreover, is as acute a political instrument as any. When Europeans began introducing Chinese subjects to their nascent cinema in the late nineteenth century, the protagonists were typically heroic and often imperialistic European types. But the Chinese, as Hollywood and Hearst contrived them, were absurd and abject "Orientals," mere caricatures composed of the obvious symbols—pigtails, opium pipes, and long fingernails; their behavior was bounded by corruption and intrigue. By the time of the May Fourth Movement of 1919 young Chinese intellectuals began to create films about their own liberated lives and became avid patrons of the foreign films that regularly appeared in major cities. By the late 1920s several Chinese studios, most of them in Shanghai, were producing films on the same themes that prevailed in left-wing and experimental literature and in activist politics: protest against foreign imperialism, investigation of labor organization, sexual romance, and the promotion of women's rights. The Manchurian Incident of 1931, which signalled the rise of Japanese imperialism, divided the cinema world between entertainment films, financed by Chinese and foreign capitalists in adopted Hollywood style, and radical films, made by Communists and other leftists determined to arouse popular will to resist Japan and to criticize any Kuomintang acquiescence in its imperialistic schemes.

Leyda left China just when the forces of cultural warfare were being aligned under the aegis of Chiang Ch'ing, whose supreme cultural agency behind the scenes Leyda already sensed. Within two years all film production and screenings ground to a halt, not to be resumed until the late

1960s. Success in the production of politically approved films is still meager.

Leyda's ambitious research and reports should open to both Sinologists and comparativists new avenues of social and cultural history. Photographic illustrations enliven certain of the descriptive passages, though foreign research on Chinese films will continue to be thwarted in the foreseeable future by the near impossibility of seeing Chinese films for research and/or entertainment.

Poor technical and editorial service from MIT Press curtail the book's general utility. Unindented paragraphs make for tedious reading and difficulty in distinguishing the author's narration from long quotations. Inevitably, factual errors arise in a book by an author who is alienated from the language of his subjects, and who is seeking information at a time of political turbulence, when candor about the past and present carries little premium. Among the more egregious errors is his claim that the film actress, Ch'en Bo-erh (Ch'en Po-erh in the usual Wade-Giles transliteration) was minister of culture in the first government of the People's Republic of China in 1949. Official Chinese records show that that post went to Mao Tun, a figure of far greater stature and longer standing in the history of modern Chinese culture.

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EDWARD R. BEAUCHAMP. *An American Teacher in Early Meiji Japan*. (Asian Studies at Hawaii, number 17.) Honolulu: Asian Studies Program, University of Hawaii, University Press of Hawaii. 1976. Pp. xiii, 154.

William Elliot Griffis (1843–1928) taught in Japan for three years as a young man and then returned to his native United States and a career which, among other things, made him his generation's most prolific interpreter of Japan. Edward R. Beauchamp, a specialist in the history of American teachers abroad, builds this informative analysis of Griffis' relations with Japan through close reference to his copious letters, diaries, and published works.

Griffis, a deeply pious Christian, felt in retrospect that fate had prepared him for a life of contact with Japan. When, therefore, the missionary-educator Guido Verbeck sent a request for a foreign teacher through church channels, Griffis could easily interpret the offer as a call from God to interrupt his theological studies for service in Japan. He accepted and found himself in 1871 teaching in the small domain capital of Fukui on the north shore of western Honshu.

He established an ambitious school in Fukui but in 1872 moved to Tokyo, where he mingled with

the illustrious foreigners and Japanese who were intent, as he was, on the rapid modernization of Japan. In 1874 he returned to theological studies and a number of pastorates. During these years he frequently wrote and spoke about Japan; after retirement at age sixty he devoted his full energies to the subject. The Japanese government rewarded his labors with an Imperial decoration and a tour through the Japanese empire. His greatest single work was *The Mikado's Empire*, which appeared shortly after he returned from Japan and went through many reprintings. Numerous later works included biographies of several Americans who had worked in Japan.

Overattention to sources with a resulting lack of analysis makes Beauchamp's Griffis seem incomplete. More attention to him as a representative of the missionary personality could have told us much about the kinds of Americans who so involved themselves in Japan's modernization. Less attention to Griffis' concerns over money and loneliness along with more extensive coverage of his biographies of other foreign advisors (they are only mentioned) might have better explained Griffis' reputation in contemporary America. Let us hope Beauchamp plans more ample discussion of these themes at a later date now that he has introduced this important topic.

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LINCOLN LI. *The Japanese Army in North China, 1937-1941: Problems of Political and Economic Control.* (East Asian Historical Monographs.) New York: Oxford University Press. 1975. Pp. 278. \$21.50.

At a time when their memory of Sino-Japanese War experiences may have a profound effect on Chinese leaders' attitudes toward Japan, Sino-Japanese trade, and detente between China and the American-Japanese alliance, this gem of a book provides an essential historical perspective for students of contemporary China.

This work focuses on the problems and techniques of Japanese political and economic control of North China between July 1937 and December 1941. As a secondary objective, the author sets out to relate Japanese success (and failure) to the rise of Chinese Communist party influence.

In a thoroughly rigorous organization of his material, Lincoln Li sets the stage for the reader in an introduction and a broad analysis of Japan's pre-war China policy. Six chapters follow, on Political Developments to December 1937, Policy and Administrative Control, Ideological Control, Economic Control, Rural Economic Control, and Problems of Internal Security. A final chapter on the Deterioration of Japanese Control after December 1941 portrays the erosion of Japanese mili-

tary and political power marked by the expansion of Communist influence.

Drawing on an impressive collection of official Japanese and Chinese sources, both Nationalist and Communist, plus excellent unpublished sources such as Lee Ngok's Ph.D. thesis on Chinese Communist bases in North China (1938-43), the author provides a clear insight into the dilemmas that confronted the Japanese leadership in North China. Li states that "the survival of Chinese resistance was guaranteed by the geographical extent and the size of China's population, by the basic manpower shortage of the invading army, by the inability and unwillingness of the Japanese to supplement this shortage with armed Chinese manpower, and by their continuous distrust of Chinese collaborators."

Japanese political ambitions in North China expanded to pace the early and rapid buildup of military strength (10,000 in July 1937 to 200,000 by September 1937). Both the coming Pacific War and the slow progress of the war in Central China imposed ceilings on North China troop strength and forced the Japanese to experiment with a variety of political and economic control techniques. The shift from a strategy of frontal military assault before October 1938 to a strategy of siege thereafter was almost foreordained. The story is instructive for historians and contemporary policy planners alike.

WILLIAM WHITSON  
Library of Congress

EDWARD REYNOLDS WRIGHT, editor. *Korean Policies in Transition.* Seattle: University of Washington Press. 1975. Pp. xii, 399. \$12.50.

This collection of studies by Edward Reynolds Wright and ten other scholars is designed to present comprehensive views of past and present patterns of political behavior of Korean institutions and individuals. It consists of a survey of political history, focusing on the 1948-74 period, followed by chapters analyzing South Korea's constitution, political structure, and party politics, the role that students and the military played as "major contributors" to Korean politics, and the impact of the policy and leadership of the government on economic development and foreign affairs. Also included are two interpretive articles on the persistent traditional and institutionalized patterns of political behavior in Korean society, which had experienced serious social disintegration. One of the studies examines current theses on Korean political patterns, and proposes a basis not only for a theoretical interpretation of Korean political behavior, but also for the Korean way of life, while promoting a broader and more accurate interpretation of past and present Korean political cultures.

The book achieves more than the modestly stated objective of providing "an introductory basis for understanding Korean politics." While providing a sound basis for a greater knowledge and deeper understanding of Korean politics from the standpoints of traditional culture and social patterns, and historical experiences, it reveals the difficulty, if not impossibility, of establishing an effective representative and democratic system of government patterned after that of the Western democracies. It also points out the need for ideological, psychological, and sociological readjustments for the sake of beneficial reconciliation between the old and new political forces. The authors' scientific methodology, objectivity, honesty, and serious scholarship are evident throughout the book. It has a good selected bibliography of books in English on South Korean politics and a good index.

I found no factual errors in the volume, and the authors' views are sound. I feel, however, that as a group the authors have treated the subject less critically than might have been desired; in some respects the articles read more like the work of apologists rather than of scholars seeking to identify the unique characteristics of political patterns of contemporary Korea. The avoidance of value judgment and the absence of studies on local and regional politics are conspicuous. The value of the book would have been enhanced had the authors included the perceptions of political leaders regarding their role, the people's expectations of their government and political leaders, and biographical studies of key leaders and their political ideologies and beliefs.

These criticisms notwithstanding, the book will make a significant contribution toward the objective understanding of the nature of Korean politics in transition. This long-awaited book will be valuable not only to Korea specialists, but also to those in East Asian and comparative studies in the government and politics of newly developing nations with long histories.

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DAE-SOOK SUH and CHAE-JIN LEE, editors. *Political Leadership in Korea*. (Publications on Asia of the Institute for Comparative and Foreign Area Studies, number 27.) Seattle: University of Washington Press. 1976. Pp. xvi, 272. \$9.50.

This work is the result of two symposia on Korean political leadership held in Seoul during the summers of 1971 and 1972. It comprises eight papers and an appendix.

James B. Palais, "Political Leadership in the Yi Dynasty," is an excellent encapsulated survey of

kingship during that dynasty. The section "Bureaucratic Leaders in a Period of Change, 1876-1910" is highly recommended. One must question, however, some contradictory statements: "Their (the Korean kings') traditional legitimacy was relatively weak. . . ." (p. 4); and "There was little talk of constitutional monarchy or republicanism, for the traditional legitimacy of the monarch was still strong and respected" (p. 22).

Chong Lim Kim and Byong-kyu Woo, "Legislative Leadership and Democratic Development," is an interesting statistical study that demonstrates an inverse correlation between education and commitment to majority rule and minority rights. The sampling did not raise the questions of age or social background that would have helped explain deviation from expected results according to Western norms.

Bae-ho Hahn and Ha-ryong Kim, "Party Bureaucrats and Party Development," provides evidence that there is little likelihood of the emergence of strong political parties in the presence of a strong administration that is not derived from a political base. Dong-suh Bark and Chae-jin Lee, "Bureaucratic Elite and Development Orientations," meticulously tabulates the data for a single senior bureaucratic rank, Grade II A. The authors clearly indicate the limitations of their data. This bureaucratic group espouses a positive, confident, and futuristic orientation. Sung-chik Hong and Young Ho Lee, "Popular Perception of Political Leadership," purports to give the opinion of Korean society based on 898 highschool students, 413 university students, 235 farmers, 165 journalists, and 33 legislative aides! This is hardly a representative sample of the Korean population.

Dae-sook Suh, "Communist Party Leadership" examines the composition of the Central Committee and its Political Committee and Standing Committee in the five Workers' party of Korea congresses (1946, 1948, 1956, 1961, and 1970). Of the five elite factions identified, only the Partisan Group that participated in guerrilla activities with Kim Il-sŏng in Manchuria was consistently reelected to the Central Committee. By 1961 this group had gained control of the party. Neither the initial Soviet military presence nor the later Chinese military presence in Korea had any significant influence on the top leadership of the party. With the Fifth Party Congress of 1970 there is a significant increase of the "New Group," leaders trained by the Partisan Group, which seems to suggest the disappearance of factional groups in the leadership of the North.

Chong-sik Lee, "The 1972 Constitution and Top Communist Leaders," is an analysis showing that the constitution of the Democratic People's Republic of Korea was revised to reflect the reality of the consolidation of power around and in Presi-



dent Kim Il-sŏng, legitimizing in 1972 his position as chief of state, head of the party, and commander of the military establishment. The top echelon is composed of fifteen men who have been associated with him for forty years, beginning as guerrillas in Manchuria. The pattern of personnel assignment in the power structure reveals increased functional specialization.

Glenn D. Paige, "Toward a Theory of Korean Political Leadership Behavior," does not examine the specifics of Korean political leadership, but is a discussion of methodological approaches to the study of North and South Korea "separately," then "comparatively" with other systems, and finally "universally" to draw insights from Korean experience "for understanding political leadership anywhere."

The appendix, Glenn D. Paige, "Toward a Political Leadership Profile for a Changing Society," does not deal with Korea, but rather with the construction of models and hypotheses for the study and analysis of political leadership.

As one has come to expect of such collectanea, the quality and scholarship are uneven. While the papers as a whole contribute to our understanding of Korean political leadership, Palais' paper and the two papers on Northern Korean leadership are particularly enlightening. Since the symposia were held in Seoul, there is an understandable absence of any discussion of the presidency of the Republic of Korea.

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ROBERT R. SIMMONS. *The Strained Alliance: Peking, P'yŏngyang, Moscow and the Politics of the Korean Civil War*. New York: Free Press. 1975. Pp. xxiii, 287. \$10.95.

The major emphasis of this book concerns the thesis that although there was an alliance between North Korea, China, and the Soviet Union during the Korean War, diplomatic relations among the three nations were less than cordial. While Robert R. Simmons examines the history of the peninsula prior to the outbreak of hostilities, most of his research deals with the critical years 1950-53. Relying heavily on declassified United States military data, as well as radio broadcasts and newspaper editorials from P'yŏngyang, Peking, and Moscow, the author contends that the coalition members seldom coordinated their efforts throughout the conflict. Early signs of duplicity were evident before the strife began, when the Soviet Union vacillated on using its influence to get the Chinese regime seated in the United Nations.

While the "Strained Alliance" suggests a division within the three powers, the real split was between Moscow and Peking. Stalin openly en-

couraged North Korea and China to resist United States and South Korean forces, but his real fear was possible American involvement in Siberia. The Chinese People's Volunteer Army entered the war to protect the Manchurian border, and quickly became disillusioned by the Soviet refusal to supply the CVPA with the modern weapons necessary to meet successfully the challenge of the United Nations armies. North Korea, needing the support of both powers, was obliged to adopt a conciliatory policy with its two neighbors in order to retain their support.

Simmons worked under a severe handicap: primary source material was impossible to obtain. Nonetheless, his research is comprehensive, and the bibliographical effort is more than adequate. This book will be a valuable addition to scholars specializing in modern Asian studies.

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J. F. RICHARDS. *Mughal Administration in Golconda*. London: Oxford University Press. 1975. Pp. xiv, 350. \$26.00.

In 1686-87 the South Indian Sultanates of Bijapur and Golconda fell to the invading armies of Awrangzeb, bringing more of the subcontinent under the domination of a single ruler than on any previous occasion. Yet within a quarter of a century Awrangzeb and his successor would be dead, and from 1712 onwards the decline of the Mughal Empire would be swift and seemingly inexorable. Traditionally historians have tended to link the two processes, assuming that the efforts required to eliminate the two Deccani Sultanates so stretched the empire's resources in terms of manpower and revenue that they contributed very considerably to the subsequent collapse of the imperial system.

J. F. Richards, in one of the most important studies published so far on the Mughal period, makes no pretence to a revisionist approach to the crucial years 1687-1725. Instead, he provides a meticulously researched, intuitively perceptive interpretation of the way Awrangzeb's officials endeavored to introduce standard Mughal administrative practice into Golconda, and of the way successive governors—reflecting parallel developments elsewhere in the empire—acquired an appetite for *de facto* autonomy. This would lead, after 1725, to the emergence of the Asaf Jahi state of Hyderabad.

The failure of Mughal rule in Golconda—the failure to maintain law and order in the countryside, to conciliate the Telugu *nayaks*, and to prevent a decline in agricultural productivity and state revenue—exemplified in a regional setting weaknesses which bedeviled the whole empire dur-



ing the last decades of the seventeenth century. In his detailed analysis of what went wrong in Golconda, Richards touches on the heart of the problem of Mughal decline in this period. The system was still functioning, the bureaucratic structure remained intact, and individual officers could still demonstrate remarkable energy and perseverance in the imperial service. What was lacking—more than money, far more than loyalty—was leadership from above, and this was true even of Aurangzeb's last years. After 1712 leadership at the top might still have worked wonders. The tragedy for the survival of Mughal rule was that it was patently not there.

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KENNETH W. JONES. *Arya Dharm: Hindu Consciousness in Nineteenth-Century Punjab*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xvi, 343. \$14.00.

SUFIA AHMED. *Muslim Community in Bengal, 1884-1912*. Dacca: Published by the author; distributed by Oxford University Press, Bangladesh. 1974. Pp. viii, 425.

For the past two decades regional studies have been rapidly enriching our knowledge of how modern India developed. They have shown us how unevenly cultural and political movements grew in different parts of the subcontinent and then how they fed into nationwide currents in the twentieth century. These two books are further contributions to this expanding field, the first by Kenneth W. Jones an excellent work of long-term value, the second by Sufia Ahmed a pedestrian but useful volume.

Jones has labored long to produce a detailed and fascinating study of the genesis and development of the Arya Samaj in the Punjab through the late nineteenth century and its connections to nationalist and Hindu communalist movements in the twentieth century. He also sketches the relationships of the Samaj to all the other important cultural movements, Hindu and Muslim, at work in the area. He bases his book on extensive use of primary sources, writes with admirable clarity and detachment, and has produced the best work we have on the Samaj.

My one quarrel with the author is over his sloppy use of some terms. He assumes that men involved in these modernizing-cum-revivalist movements are "alienated and marginal men." He never tells us what he means by alienated and whether the term has psychological, sociological, or cultural significance. He also talks of "levels" whenever he has two or more phenomena present;

it would be better to talk of levels of a political or cultural system in a more specific way.

Ahmed has published her own 1960 London University thesis with nary a change. It contains a good deal of information about the lag of the Bengali Muslims behind their Hindu neighbors and their resentments about this. But the book is merely a catalogue organized into the broadest categories: educational development, economic condition, social and political activities, and Bengali writings. The single chapter with a more concentrated focus is on the partition of Bengal. There is hardly any effort at analysis of the data, which is not presented in a way that gives a sense of change and development through her chosen period.

The author identifies so strongly with the Muslims that she relives the period in partisan fashion. She blames the Hindus for almost every deficiency of the Muslims, though she occasionally notes the internal divisiveness and lack of drive within the Muslim community. She accepts all Muslim and British charges against the Bengal Hindus at face value, but dismisses any Hindu responses to such charges. An example of this is her version of the 1905 partition of Bengal. Whereas John McLane and Z. H. Zaidi have convincingly demonstrated the political motives involved, Ahmed never mentions them.

Jones has given us a volume that will be of great interest to all students of modern Indian history and religion; Ahmed has presented a summary of many relevant documents that classifies information but never stretches our minds.

LEONARD A. GORDON  
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C. A. BAYLY. *The Local Roots of Indian Politics: Allahabad, 1880-1920*. New York: Oxford University Press. 1975. Pp. xvii, 314. \$32.00.

The Indian National Congress and its relation to political change is a ubiquitous topic in historical literature on modern South Asia. It has been considered as a congregation of Western-educated, middle-class nationalists, as a Hindu-dominated political association, as a mass movement, and more recently in its regional manifestations. Now C. A. Bayly has produced a monograph which achieves a noteworthy level of conceptual sophistication and intensive research in this sphere, albeit without stylistic clarity. He convincingly argues "that political change in northern India between 1880 and 1920 can be fruitfully approached through a detailed examination of the alliances and dissidences of groups in local society" (p. 271). His microcosm is Allahabad, an area with a considerable cultural and political heritage for Hindus, Muslims, the British, and even-

tually the Indian National Congress. To lay a solid foundation for his thesis, Bayly devotes the first third of his study to an elaborate delineation of Allahabad Municipality—a loosely organized political unit situated at the strategic and sacred confluence of the Ganges and Jumna Rivers—its social groups, and its political and communal organizations. Though he gives some attention to Eurasian railway workers and Indian artisans, his primary focus is on the elites of European officials and businessmen, Indian service and professional people, landholders, commercial magnates, and communal leaders. The remainder of his book is a broadly conceived chronological analysis of political change and the development of the Indian National Congress in a specific geographical arena.

According to Bayly, the decisive spurts of growth in depth of leadership and range of activities which the Congress in Allahabad experienced during the late 1880s and again in the late 1910s were caused by the response of local elites to their own problems rather than the impact of outside leaders and events. Thus the British drive in the 1880s for increased revenues and administrative efficiency pushed threatened commercial magnates and service communities to join the Congress to secure a wider platform of protest. Conversely, when Allahabad elites thought that their interests could be best secured through cooperation with the Government of India and its local incarnation, they staunchly resisted appeals for boycott of that Government even when delivered by such prominent nationalists as B. G. Tilak during such momentous periods as the feverish agitation over the partition of Bengal in 1907.

Besides the obvious one of interaction between national and parochial politics, the key themes in this analysis are the continuity of cultural forms, political structures, and social groupings; the pivotal role of Government as the distributor of wealth and patronage; the greater importance of vertical linkages such as those between *rais* or notable and publicist over the horizontal ones of class, caste, and community in influencing political alliances and activities; and the absence of any genuine influx of the masses into politics during the early 1920s. Thus Bayly endorses and further substantiates motifs current in historical research on South Asia in many academic centers. His study is significant for his careful integration of these diverse themes in a particular locality, for his special concern with ties between commercial leaders and politicians, and for his perceptive use of extensive sources ranging from governmental documents to English and vernacular newspapers, to private papers, and to personal interviews.

Two problems are evident in Bayly's work which reflect the general orientation of the so-called

Cambridge school of South Asian history. First, his emphasis on social origins and economic interests as the major determinants of political activities creates rigid categories which slight those who are exceptions. Tej Bahadur Sapru, a Kashmiri Brahmin like the Nehrus, chose the role of cautious intermediary just when Motilal Nehru accepted the boycott of the expanded Legislative Councils in 1920. Bayly does not attempt to explain this difference and gives no evidence of having utilized Sapru's papers. Second, this approach seems to imply that human actions are normally rational and consistent and thereby reduces the complexity of human motivation. Still, Bayly does acknowledge that ideology influences men, and his study is one of the most balanced of those recently published by the Cambridge group.

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SARVEPALLI GOPAL. *Jawaharlal Nehru: A Biography*. Volume 1, 1889-1947. Cambridge, Mass.: Harvard University Press. 1976. Pp. 398. \$17.50.

The material both on and by Nehru is very considerable; several biographies of him have already appeared. Yet in this volume Sarvepalli Gopal has managed to reduce the vast array of materials into a book of less than four hundred pages. Moreover, this work represents a real contribution not only to a study of Nehru but also to the social and intellectual history of modern India. Compared to Gopal's works on Ripon, Irwin, and on British policy toward India, this volume is like a breath of fresh air that is bound to change the perspective of those who look at the period under consideration, particularly the years from 1920 to 1947.

In writing a biography an author is always in danger of being seductively drawn to the memorabilia of that individual and dwelling on his or her personal contribution in a narrow way. In this case the pull must have been very great, for of all Indian public figures the personal material on Jawaharlal Nehru is perhaps the most substantial. This is partly because he was in the eye not only of the Indian public but of a much wider community in England and elsewhere from rather early in life. The police in India were fascinated by Nehru. Nehru himself wrote a great deal pertaining to his personal life. He also wrote many letters to his father and to others. By great good fortune, many of these documents remain, and Gopal has consulted them. But he has resisted the temptation to be mesmerised by Nehru and has focused more squarely on the social, political, and intellectual issues raised by the course of Nehru's life. Thus, for instance, he examines the economic position of the peasants of the United Provinces in this quarter-century and also the problems facing the Con-

gress, not only there but nationally and in many localities as well.

Gopal has studied and evaluated British attitudes toward the course of Nehru's politics. He has a lively sense of the conflicting pulls within the British administrative machine in India and mercilessly exposes maladroitness moves with humor and precision. What is even more delightful is his ability to analyze the middle-class milieu out of which Nehru and others like him came. The expression of these opinions is bound to create a few enemies, yet it is this honesty which will assure Gopal's book an important place in the growing literature on the period during which the British gradually relinquished the reins of control over India.

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HUGH TINKER. *Separate and Unequal: India and the Indians in the British Commonwealth, 1920-1950*. Vancouver: University of British Columbia Press. 1976. Pp. 460. \$18.50.

This study on overseas Indians in the British Commonwealth concentrates on their political status and the attempts to gain for them a measure of equality within the countries and colonies of their domicile. The author seeks to "establish how far the overseas Indian communities had their role and future conditioned for them by the decisions of their rulers."

In a chronological narrative, to which he appends a useful page-keyed chart of important events, Hugh Tinker offers a judicious analysis of the three-sided issues between India, London, and the other Commonwealth constituents. Antipathies between nationalists and British officials in India, between the Colonial and India Offices in London, and among the racially divided populations of the colonies and dominions complicated these issues. The frustration of efforts to afford Indians equality in their new homelands was almost guaranteed by the willingness of colonial officials to see the matter as "the Indian problem" rather than as a problem of assimilation, adaptation, and development within the host society.

Overseas Indians' conditions gave rise to Edwin Montagu's initiatives in 1921 to provide India with an identity and an apparatus for conduct of external affairs. From 1920 through 1950 both British and national governments of India endeavored to find means for intervention on behalf of Indians in the Commonwealth. In an area of developing independence in the white dominions and growing nationalism in the non-white colonies, the search was not fruitful. What India sought was access for Indians to full civil rights within the host territories. Yet the very fact of external intervention,

coupled with racist and/or indigenous nationalist dogmas, emphasized perceptions of Indians as sojourners with no stake in local societies. It is a melancholy story of good hopes dashed and of constructive opportunities lost.

Beyond issues of Indian rights lay broader questions of the nature of the Commonwealth itself. The chronological chapters survey colonies and dominions serially in relation to evolving policies and relations of the whole. Ultimately the idea of a Commonwealth citizenship offered a path for full Indian participation—as individuals and as a nation. Only in the London-centered discussion of this issue does the author's masterful narrative falter, as did the dreams of Commonwealth equality. Initiatives for good—and for ill—lay beyond London in the ever more independent ex-colonies. Historians of both India and the Commonwealth will find much of interest in Tinker's perceptive work.

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KITSIRI MALALGODA. *Buddhism in Sinhalese Society, 1750-1900: A Study of Religious Revival and Change*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xiii, 300. \$16.50.

In the past decade scholars working in the Theravāda Buddhist heartland—especially Ceylon, Burma, and Siam—have made a number of outstanding contributions to the history and sociology of religion and to social anthropology. Though Kitsiri Malalgoda's contribution to this growing corpus of works is less analytical and more modest in scope than the pioneering studies of authors like Melford Spiro, Gananath Obeyesekere, and S. J. Tambiah, *Buddhism in Sinhalese Society* adds an important dimension to the works of these and other authors. In contrast to the village and socioanthropological orientation of much of the work on Theravāda Buddhism, Malalgoda has written a basically institutional study—a history of the Sinhalese *samgha* or monastic order between about 1750 and 1900 and its responses to the challenges posed by a succession of European colonial regimes.

Beyond his fine writing style and his mastery of the relevant archival and published sources, Malalgoda's considerable success with this approach can be attributed to his sensitivity to the links between the Sinhalese monastic order and the social and political context in which that institution developed. Malalgoda explores in detail the severe problems which the collapse of the Kōṭṭe and Kandy kingdoms and the resulting loss of royal patronage posed for the Sinhalese *samgha*. He shows how the lack of political support initially meant a decline in discipline within the monastic

order and a reduction in potential sources of economic sustenance. He also demonstrates how the absence of royal backing opened the way for deep social and economic divisions within Sinhalese society to be played out in the monastic sphere. He argues that these divisions, which on the surface resulted only in the fragmentation of the monastic order and a series of rather inane theological disputes, in fact provided the basis for the revival of Theravāda Buddhism in Ceylon. They strengthened the monastic order by incorporating the non-Goyigama castes, by stimulating greater involvement on the part of laymen, and by arousing a sense of competitiveness between different monasteries and fraternities which caused the monks to be "more watchful of the behavior and religious knowledge of one another" (p. 259).

In setting forth these arguments Malalgoda also calls attention to important regional variations in Sinhalese religious development, and traces the shift in the locus of monastic activity from the Kandy heartland in the interior to the coastal lowlands and the city of Colombo. His account builds to the confrontation between Sinhalese Buddhism and the European missionaries, and he evaluates at some length the contribution of Henry Olcott and the Buddhists' Theosophist allies to the Buddhist "victory" in this struggle. Although Malalgoda's predilection for the Buddhist side is clear, his discussion of Christian tactics and activities provides fine insights into both the missionaries' strengths and the serious shortcomings of their approach.

A number of problems arise from Malalgoda's tendency to lump the Portuguese and Dutch together in the introductory sections of his study. In doing so he creates considerable confusion with regard to their quite different roles and impact and their relationships with the Kōtte and Kandy kingdoms, which are dealt with in surprisingly vague terms for this early period. Aside from these problems the major shortcoming of the work is Malalgoda's failure to relate the institutional changes that are the focus of the work to the peasants who made up the great majority of the Buddhist faithful for whom the *sangha's* revival was so critical. Wealthy lay patrons are considered at some length and Sinhalese villagers are seen cheering Buddhist orators or smirking at missionary blunders, but little serious attention is given to peasant responses to and their roles in the religious revival. Despite these minor shortcomings, this is an intelligent, sensitive, and provocative study providing both overall patterns and excellent specific insights into the process of religious change in Ceylon during the early colonial era.

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A. M. DUBINSKY. *The Far East in the Second World War: An Outline History of International Relations and National Liberation Struggle in East and South-East Asia*. Translated by V. EPSTEIN. (USSR Academy of Sciences, Institute of the Far East.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 457.

After finishing this book one wonders whether it has really been worth the effort to read through the 458-page monograph, one-third of which consists of footnotes. A. M. Dubinsky follows a Marxist-Leninist interpretation of history and offers no new evidence. He has written a survey of the Far East from India to Japan in World War II, a story we are familiar with from Western literature.

The book is full of Communist jargon and diatribes. The Western allies are invariably branded "imperialists," "reactionaries," "monopolists," and the like; the Soviet Union and its Communist allies are presented as "liberators," "progressives," and so on. The reader may survive such jargon, but he will not fail to notice the deliberate distortions and omissions.

The author characterizes U.S.-Japanese negotiations in 1941 as a U.S. plan for a Far-Eastern Munich, which "failed to come to term mainly because each thought the other's appetites exorbitant." He repeats "the historic triumphs" of the Red Army against Germany and Japan without giving credit to the Lend-Lease program. He emphasizes a Japanese army buildup in Manchuria in August 1945, when Russia entered the war, to exaggerate the Soviet contribution to defeating Japan. Dubinsky attacks the Anglo-American-Chinese treaty of 1943 which enabled the Western powers to retain economic concessions and territory (Hong Kong) in China, while he makes no reference to the territories Russia wrested from China in the nineteenth century. He not only fails to elaborate on the German-Soviet Non-Aggression Pact, one of the sinister agreements, but also glosses over the Japanese-Soviet Neutrality Pact as something in which only Japan was interested. Finally, he claims Russian entry into the war against Japan was dictated by concern for the security of its Far-Eastern border.

The author's stated purpose is to present "the contribution the Soviet Union made to the struggle of the peoples of East and Southeast Asia for their independence." But the rise of independence movements is to be attributed not so much to Soviet contributions as to Japanese failure in military administration. The exception is Vietnam but even there, any direct Soviet contribution to Ho Chi Minh's national liberation movement is doubtful. Soviet support to the independence movements was indirect, if indeed there was any.

There are numerous errors. For example, the Japanese did not move administrative bodies in the fall of 1943 from Sumatra to Java to achieve



improved administration of Malaya. Hatta was not a "man of pro-Japanese sympathy." There are also misprints and misspellings and inconsistent spellings of personal names.

A more serious flaw is bibliographical. Dubinsky, though familiar with some English and Japanese materials, fails to utilize works published outside the Soviet Union in more recent years. Excepting a few sources, the materials that the author consulted were published in wartime and in the late 1940s and 50s. He claims that the work draws primarily on information available in Soviet archives, but he simply cites "Soviet Historical Archives," "Soviet Diplomatic Archives," and "Soviet Military Archives" without further information.

There are merits, however, in the book. It is refreshing to read a Soviet version of World War II and the Russian contributions. The reader may well benefit from the listing of a large number of Soviet studies on the subject in the bibliography. Nonetheless, the book is a disappointment as a measure of Soviet standards of scholarship.

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WILLIAM J. DUIKER. *The Rise of Nationalism in Vietnam, 1900-1941*. Ithaca: Cornell University Press. 1976. Pp. 313. \$15.00.

What should have been common knowledge, but alas was not, namely that the Vietnamese Communist movement did not emerge from nothingness, is ably demonstrated in this volume. *The Rise of Nationalism in Vietnam, 1900-1941* offers a comprehensive historical background to the two horrendous Indochinese wars of the last generation. William J. Duiker carries the story as far as the founding of the Vietminh (Vietnamese Independence League) in May 1941, thereby providing substantial information on the crucial fifteen years after the mid-1920s, the termination point of the other major study of late nineteenth- and early twentieth-century Vietnamese nationalism, David Marr's *Vietnamese Anticolonialism, 1885-1925*. Duiker also gives more data on Communist predecessors and nationalist rivals than does John McAlister's *Vietnam: The Origins of Revolution*, which focuses on the Communists, and he nicely supplements the more general studies of Alexander Woodside, Joseph Buttinger, Helen Lamb, Ralph Smith, and others.

Duiker, however, does not break much ground not already covered in these studies. Nor does he give as penetrating an analysis of the earlier opposition to the French as Marr, or of the Communists as McAlister. But in pulling together many strands and carrying them forward to the ascendancy of the Communists, he effectively supports

his important conclusion that Vietnamese Communism was "born out of the nationalist movement" (p. 285) and in turn "took over the mantle of nationalism in Vietnam" (p. 290). It is unfortunate, to say the least, that our policy-makers did not understand this connection twenty years ago, and also unfortunate that more English-speaking historians did not take up Vietnamese studies in time to argue this conclusion.

There are some questionable passages. Duiker speaks, for example, of Vietnam's having only itself to blame for its "inability to stem the tide of French advance" in the nineteenth century, as if other third-world countries had been able to do so. Later he follows the frequent tendency to over-stress foreign, especially Chinese and French, influences on Vietnamese patriots, without adequate attention to their indigenous roots. Among other points, one would like to know what happened to the Trotskyists, who, we are told, were the dominant group in Saigon in 1939 (p. 253).

Despite such minor problems, in showing the deep roots of anti-imperialism in Vietnam and that "it was first and foremost the communists who made the effort to transform a nationalism into a mass phenomenon" (p. 290), Duiker has contributed a valuable study.

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S. J. TAMBIAH. *World Conqueror and World Renouncer: A Study of Buddhism and Polity in Thailand against a Historical Background*. (Cambridge Studies in Social Anthropology, number 15.) New York: Cambridge University Press. 1976. Pp. viii, 557. \$37.50.

S. J. Tambiah's study of Buddhism and polity in Thailand is a complex, but uneven work. The book opens with an exposition of the classic concepts of Buddhist kingship and cosmology, comparing them with Brahmanical ideas. Thai Buddhism and polity are treated in terms of this classic model. The reform movements of the nineteenth century and the Sangha Acts of the twentieth are also covered. The second part of the book, an account of contemporary relations between the state and the Sangha, is based on research in Thailand.

Tambiah is at his best when dealing directly with primary sources, classic Buddhist texts, and his own research. Difficulties appear, however, in the connective sections between his discussion of the classic Buddhist concepts of kingship and polity and his description of modern practices. The theoretical concepts by which he seeks to relate the opening and the closing chapters of his book, his galactic (concentric circles) and radial (center-dominated) polities, do not provide an adequate



description of the way in which Thai society functioned in the past, nor of the way in which it deals with the present. Little difference exists between the two models: the galactic polity possesses centrist elements—the degree of local autonomy is overstated—while the modern radial/centrist state has decentralized some of its functions, supporting regional economic and educational centers.

The absence of references to Thai-language materials, particularly those concerned with Thai history and the Thai practice of Buddhism, poses additional problems. The classic Buddhist texts do provide a model for Thai polity. But was that model followed in all of its aspects? Might not there have been reinterpretation of the classic texts in accord with local society and local traditions, including pre-Buddhist ones?

Tambiah's study mentions only a single Thai source, the Pali *Jinakalamali* of Chiangmai. There are no references to the corpus of Buddhist literature produced in Chiangmai or in other Buddhist centers in Thailand. This is unfortunate, especially since much of this material is in print: historical chronicles (*lamnan* and *phongsawadan*), temple histories, inscriptions, biographies of religious leaders, Buddhist sermons, and commentaries. No definitive study of Thai Buddhism can be written without the use of these materials.

Tambiah is strangely reluctant to consult the work of many younger scholars, among them, Janice Stargardt's study of Pagan, I. Mabbett's interpretation of the *Devaraja* cult of Angkor, Charnvit Kasetsiri's thesis on Ayutthaya, and E. T. Flood's thesis on Japanese-Thai relations in the 1930s. Recent research, such as Michael Aung Thwin's study on Pagan, which presents an alternative theory regarding monarchical attempts to purify the Sangha, and Nidhi Aeusrivongse's article on ancestor worship at Angkor, is moving in quite different directions from Tambiah's work by stressing indigenous/local aspects of kingship and polity. Both approaches, the study of classic texts and the study of local traditions, need to be taken into account in the study of Southeast Asia.

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LEONARD Y. ANDAYA. *The Kingdom of Johor, 1641–1728*. London: Oxford University Press. 1975. Pp. xvii, 394. \$35.50.

An alliance with the Dutch in the capture of Malacca from the Portuguese in 1641 gave Johor the chance to become the predominant Malay power in the Straits. The conquest of Johor by a Minangkabau pretender to the sultanate in 1718 destroyed its authority.

This is a history of the kingdom as a political institution, not of the countries subject to its au-

thority. The facts are almost all derived from Dutch records, though the Malay chronicles provide orientation for interpreting them. The author demonstrates, however, that history based on European records need not be Europe-centered. He keeps the spotlight on the shifting center of government, its officers and notables, and the people—mainly *Orang Laut* (sea Malays), who gave effect to its policies. A part of the kingdom (such as Siak) is illuminated when it receives political attention from Johor, then falls back into the shadows.

A pioneering work such as this must be chronological, but to be memorable and therefore useful it must string events on a coherent line of narrative with credible actors. Here Tun Abdul Jamil, the major statesman of the period after 1641, appears as a rational man in an intelligible institutional setting, in negotiation with the Dutch and at war against Jambi. Leonard Y. Andaya maintains a line of narrative through the years of internal stress after the murder of Sultan Mahmud Syah in 1699 and uses the narrative to illustrate the political system.

The period 1718–28 was the beginning of a new era, full treatment of which in the manner of this book would require a switch of the spotlight to the Minangkabau and Buginese polities.

The author is very conscious of the political importance of trade and of the political nursing it needed. He indicates its vicissitudes where they are relevant, but discusses only incidentally the institutions and methods by which it was developed and exploited. He makes a useful distinction between “directed piracy”—which was an instrument of commercial policy—and “undirected piracy” which negated it. Entrepot trade in the Lingga-Riau archipelago, which was an annoyance to the Dutch in the Buginese period, had evidently been tolerated in the earlier period when it was controlled by a friendly Johor. The alternative was undirected piracy that threatened the provisioning of Malacca.

Southeast Asia has been politically fragmented, but there are common features in the various traditional societies. This book treats only a small fragment, but the student of Southeast Asia will feel at home and will find many suggestive hints and questions that are incidental to the narrative.

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GEOFFREY BLAINY. *Triumph of the Nomads: A History of Aboriginal Australia*. Woodstock, N.Y.: Overlook Press. 1976. Pp. x, 285. \$15.00.

Newly appointed Ernest Scott Professor in Melbourne, Geoffrey Blainey confirms his considerable reputation with yet another brilliant book. In this history of Australia prior to white settle-

ment in 1788, Blainey has distilled the latest scholarship along with published primary materials (but has apparently rejected use of interviews with present-day tribes) into a book which asserts that the aborigines triumphed over changing climate, shoreline, food supply, and vegetation. More importantly, he takes the rather fixed and static sociological and anthropological model of the structure of aboriginal life and explains in historical terms how periodic changes were made in that model. He accepts the findings of the Lake Mungo diggings which in 1969 established the existence of aborigines in Australia at least 30,000, and possibly as many as 38,000, years ago. He makes much of this time-scale, because, as the last Ice Age ended, the shoreline of present-day Australia shrank by one-seventh as the seas rose, and Tasmania and New Guinea were separated.

Blainey discusses several theories regarding their origins, but inclines to the one that has the aborigines, as brave seafarers, coming across the stepping-stones from South East Asia before the glacial age ended. They discovered an empty continent, populated it, and adjusted to the naturally changing environment, and further modified vegetation by the use of fire. (One whole chapter is devoted to the fire stick used to burn off vegetation before the rains came.) They had no homes, crops, or woolsheds, and smoke was ever-present in the Australian air; "fire was central to their life, affecting nearly every activity" (p. 71). Blainey clearly implies that there were no tight physical boundaries, but much tribal migration. Moreover, he shows warfare as more deadly and losses more severe than most current scholarship indicates.

He scrutinizes many theories about questions such as why the giant marsupial became extinct, or what happened to the Tasmanian devil and the huge moa bird. Blainey makes constant analogies to present conditions; some of these are daring, some startling. For example: "If we specify the main ingredients of a good standard of living as food, health, shelter and warmth, the average aboriginal was probably as well off as the average European in 1800" (p. 225). His chapter "The Hunters" tells much about the skillful use of the spear, the boomerang, and the waddy. The variety of diet—from an enormous number of plants, to ducks, bird's eggs, fish, shellfish, Bogong moths, and marsupials—causes Blainey to disprove the conclusions of early explorers that the aborigine was exclusively carnivorous. Their sophisticated trapping equipment was made of wood, bones, and fiber; their eyesight and sense of smell amazingly keen; and their mimicry of bird and animal life eerily lifelike.

Blainey speaks of their use of herbs with medicinal properties, and he makes much of abortion and infanticide because "in a nomadic society only one

child-in-arms was manageable" (p. 96). Furthermore, he states that no animal milk was available in Australia. The chapter "Medicines and Drugs, Liquids and Cosmetics" is true vintage Blainey. "Cosmetics," he writes, "gave a wonderful amount of pleasure" (p. 182). This is typical of his understated, impish sense of humor. There are four excellent maps, but, alas, no photographs.

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## UNITED STATES

GABRIEL KOLKO. *Main Currents in Modern American History*. New York: Harper and Row, 1976. Pp. xi, 433. \$15.00.

Gabriel Kolko's *Main Currents in Modern American History* is a political tract presented in the guise of historical analysis. Its animating spirit is the author's conviction that the United States is an irreparably sick society. The villain is "an all-subsuming capitalism." And his historical analysis is devoted to showing "the limits of capitalism and its social order in dealing with its emerging difficulties" (p. ix).

The crux of what Kolko views as the fundamental "structural" defect of American capitalism is that its inequitable distribution of income and wealth results in the lack of sufficient consumer demand to absorb the output of the existing productive facilities, much less to provide profitable investment opportunities for surplus capital. Domestic efforts to resolve this dilemma—whether by business resorting to governmental action to impose stabilization and integration during the Progressive era and the New Deal or by the high military spending in the post-World War II years—have provided no more than temporary relief. The present recession showed that "the capitalist economy's traditional nemesis of inadequate demand and overexpansion had reappeared in almost classic form despite all the vast means that had been employed to counteract them" (p. 339).

The inadequacy—indeed, Kolko implies, the inevitable failure—of domestic solutions has left American capitalism more and more dependent for its continued profitability upon access to markets, investment opportunities, and raw materials abroad. Thus, Kolko argues, the two foundation stones of American foreign policy have been to integrate its fellow capitalist powers into a free-trade system dominated by the United States, while simultaneously resisting—covertly when possible, but increasingly through the direct application of military violence—nationalist and revolutionary socialist movements throughout the world.

Yet here too the result has been failure. In the 1970s the United States faces worsening rivalry with a resurgent Western Europe and Japan. More importantly, Vietnam has decisively shown the inability of the United States to resist the accelerating process of revolutionary change despite its resort to "terror and virtually unlimited violence" (p. 348).

Kolko's convoluted and jargon-filled prose style makes for difficult reading. And the broad outlines of his analysis are hardly new. But even the historian who does not share his premises will find much of profit in this book. Avoiding a simple-minded economic determinism, Kolko does an excellent job of delineating the conflicts among different interests over policies and tactics within the limits imposed by the dominant consensus on preserving and protecting capitalism. At the same time, he presents an illuminating analysis of how immigration, ethnic and racial divisions, geographic and occupational mobility, and government policies to guarantee relatively full employment have combined to prevent the emergence of class consciousness among the workers.

As a political tract, this work will appeal only to the already converted. Even many of those sympathetic to his indictment will be put off by Kolko's deep pessimism about the likelihood, or even possibility, of any meaningful change from within. His view of the future is a gloomy—or, to be more accurate, apocalyptic—vision of the United States "now locked into an enduring, permanent crisis at home and in the world" (p. 398).

JOHN BRAEMAN  
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Lincoln

ROBERT V. WELLS. *The Population of the British Colonies in America before 1776: A Survey of Census Data*. Princeton: Princeton University Press. 1975. Pp. xii, 342. \$18.50.

Until fairly recently social historians who were interested in the preindustrial population of early America easily served their readers by quoting Benjamin Franklin's 1755 statement that the population doubled every twenty years. So much for statistics. The rationale had an equal simplicity: the combination of the "Divine Benediction" (as one Connecticut governor put it), an "Industrious temperate Life, and Early Marriage" resulted in a naturally high birth rate. Of course, Franklin's pamphlet was a polemic designed to demonstrate to Britain the prowess of the Americans. As for "Early Marriage" in Connecticut, that too could be true. However, the 1756 census that the governor had in front of him when he penned his note on

its pastoral virtues contains no information on marriage ages. The excellent records for the town of Guilford do show an average age of twenty-six for male marriage in the first of the eighteenth century. Is twenty-six early or late? If we want more than a literary debate only comparative data can resolve this issue.

We also have Evarts B. Greene's and Virginia D. Harrington's the *American Population before the Federal Census of 1790* which contains what census material was then known as well as such contemporary estimates as Governor Shirley's belief in 1746 that Connecticut's inhabitants could be calculated at "more than one-half of" the number in Massachusetts. How does one use such diverse sources? Precisely what is needed is what Greene and Harrington consciously eschewed—a "systematic interpretation of the material."

We now have a guide in Robert V. Wells' superb *The Population of the British Colonies in America before 1776*, which is based upon 124 censuses covering twenty-one colonies for the period 1623 to 1775. The vast majority of these 68 censuses for North America and 56 for the West Indies have not been known or used as a corpus by historians. Here they are all subject to a systematic analysis dealing with household size, race, age composition, sex ratios, family size and composition, and military status. Wells' monograph commences with an introductory essay on his subject, its sources, and his general method, then covers Newfoundland and the other northern colonies, New England, the middle colonies, the South, and the islands, and concludes with two synthetic chapters dealing with colonial patterns and the composition of the household. Some of the findings indicate that by the mid-eighteenth century "fertility began to decline" in New England, New York, and even in Maryland, which had a 2.7 percent growth rate between 1704 and 1762. (In Maryland part of this increase came about from "slaves being added" to the labor supply "in addition to, rather than in place of, servants.") For New Jersey, Wells finds that the Quaker religion "was not an important factor in determining demographic behavior," thus possibly revising his earlier work on New York Quakers and the belief that the sect constituted an advanced social cohort. This book makes manifest some regional differences; for example, in Virginia in 1704, 46.9 percent of the adult males were associated with a "family" while the New England composite is in excess of 70 percent. Throughout the colonies the size of the household was at its peak when the male head of the household was between thirty-five and forty-four years of age; rich families were larger than poor ones; and households headed by blacks or Indians or women were smaller than those headed by white males. This work, rich in

comparative data, sensitive to temporal and regional variations, careful in its inferences, sophisticated in its correlations, is now the standard authority on our early demographic patterns.

JOHN J. WATERS  
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JOHN PHILLIP REID. *A Better Kind of Hatchet: Law, Trade, and Diplomacy in the Cherokee Nation during the Early Years of European Contact*. University Park: Pennsylvania State University Press. 1976. Pp. vi, 249. \$14.50.

Writing Indian history from an Indian perspective has never been a simple task, for most of the records of Indian-white relations were written by Europeans who looked upon Indians in terms of white culture. With the help of modern anthropology, however, it is possible to interpret these materials with greater insight and to understand more fully Indian society, Indian reactions to Europeans, and the cross-cultural tensions that often existed. Combining a knowledge of history, anthropology, and the law, John Phillip Reid has successfully penetrated the Cherokee mind and produced an exceptionally able history of Cherokee-white relations in late seventeenth- and early eighteenth-century South Carolina.

South Carolinians, in keeping with most other colonials, persisted in trying to fit Indian institutions into familiar European patterns. It confused the English to deal with a host of headmen from numerous Cherokee villages, and they were unable to understand that a Cherokee chief ruled by persuasion and spoke for only his own village. The English made occasional attempts to tidy up Cherokee government by creating a native "king" who would be sole negotiator between his nation and the English. Reid takes sharp issue with some other historians who have credited the English with success in this endeavor.

In an effort to avoid another disaster such as the Yamacree War, which was caused by severe trader exploitation of these Indians, South Carolina officials made consistent attempts to regulate the Indian trade and control the often unruly traders. Reid's valuable description of the often futile efforts over the years of the colony's administrators demonstrates that the well-meaning officials had not only merchant and trader interests to contend with, but Cherokee law, which differed profoundly from English common law. Neither Cherokees nor whites were able to understand each other's legal or social systems.

Approaching Cherokee history from the standpoint of its legal institutions is innovative and adds an important perspective to Cherokee studies. Reid's perceptive handling of the material and his

knowledge and interpretation of native law and its influence on Cherokee social and political structure mark this book as a major scholarly contribution.

BARBARA GRAYMONT  
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DAVID E. VAN DEVENTER. *The Emergence of Provincial New Hampshire, 1623-1741*. Baltimore: Johns Hopkins University Press. 1976. Pp. xviii, 302. \$14.50.

*The Emergence of Provincial New Hampshire* is one of the latest in a recent spate of books dealing with the socioeconomic life of New England outside of Massachusetts. Despite minor flaws, it is the best of those studies.

David E. Van Deventer's task was not an easy one. New Hampshire was settled neither by a single group nor for a single motive. For most of this period New Hampshire did not have a separate existence, the area originally being peopled by the inhabitants of four autonomous towns (Dover, Portsmouth, Exeter, and Hampton) and later governed as part of Massachusetts. Van Deventer begins his study with the founding of the initial settlements and an analysis of the origins and goals of the colonists and of the opportunities and limitations presented them by the natural environment. Part 2, "The Promise of the Land," examines the decisions reached by the settlers in their attempts to utilize the land and timber resources and the threats posed to their achievement of those goals by the proprietary controversy, Indian wars, and boundary disputes. The third part treats the rise of trade and the growing importance of a mercantile elite whose capital came from the profits of the lumber industry. Van Deventer analyzes the opportunities that existed for mercantile advancement; internal hindrances such as capital and labor shortages and inflation; external obstacles provided by wars and competition from England and Massachusetts; and the adjustments made by the merchants to circumvent those threats. In his final part Van Deventer examines the role of these economic developments in the transformation of the colony's political structure from localism to centralization, and of its societal values from communalism to individualism.

Van Deventer's research is impressive and his thirty-four tables and detailed notes provide effective evidence supporting his conclusions. He is particularly sensitive to the interaction between environment, economic aspirations, and the outside forces that impinge on those aspirations. His analysis of the relationship between those factors and social change is excellent. He overreaches himself, however, in claiming that his analysis pro-

vides the key to understanding the emergence of provincial New Hampshire's society, for while he recognizes the role of values in social change he treats the values of the colonists in a static, unimaginative manner. Yet while more attention to the processes of ideological adaptation would have made it more persuasive, *Provincial New Hampshire* should become the basic study in its field and a volume with valuable methodological implications for all scholars in colonial studies.

FRANCIS J. BREMER  
Thomas More College

JOSEPH E. ILLICK. *Colonial Pennsylvania: A History*. (A History of the American Colonies in Thirteen Volumes.) New York: Charles Scribner's Sons. 1976. Pp. xix, 359. \$15.00.

Joseph E. Illick's book is a very good attempt at a very difficult undertaking—a short scholarly history of colonial Pennsylvania. One reason that this book is a success is that the author has chosen to meet head on many of the social, political, and economic intricacies of early Pennsylvania society. He has, of course, been forced to simplify, but he has written a book to which one may confidently turn for a sound word on a number of issues recent historians have identified as important. This kind of completeness and integrity is always to be lauded in such a book, but there are from time to time unfortunate side effects. Occasionally, as one moves from a summary of one problem to that of another the pace becomes mechanical and the transitions contrived. But, in fairness, that is always hard to avoid in a survey.

If there is one underlying theme in the book it is that conveyed most clearly by the chapter headings—the gradual growth and maturation of a complex social entity. Yet the main means by which Illick has tried to convey this theme is a comparison between the intentions, values, accomplishments, and, in a word, the worlds of William Penn and Benjamin Franklin. There is strength in this juxtaposition. Some of Illick's best writing and insights are a product of this comparison, and in some ways Penn and Franklin do speak for Pennsylvania. But not completely. For neither can encompass the complexities of the society that he is to represent, and neither serves as a suitable vehicle to illustrate the actual transition from utopia to commonwealth. The weakest parts of the book are those covering the interim period between the seventeenth-century world of Penn and the mid-eighteenth century world of Franklin. And multifaceted as Franklin was, Revolutionary Pennsylvania far surpassed him. The point is that to some extent Illick sacrificed his underlying theme of the gradual growth and maturing of a complex society

and his explanation of that process to his Penn-Franklin comparison.

These few reservations aside, this is a worthwhile book, a good, solid introduction to the history of colonial Pennsylvania.

ALAN TULLY  
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LARRY R. GERLACH. *Prologue to Independence: New Jersey in the Coming of the American Revolution*. New Brunswick, N.J.: Rutgers University Press. 1976. Pp. xxi, 535. \$25.00.

New Jersey's colonial and revolutionary history has not engendered an extensive literature. Small in size, split by the circumstances of its founding and development into eastern and western sections, and dominated by its larger neighbors, Philadelphia and New York City, this little colony, caricatured as a "cask tapped at both ends," has not been mined by any of the newer schools of historiography—demographic, economic, or ideological. Yet New Jersey was the "cockpit of the Revolution," a scene of bloody civil war as well as important Anglo-American engagements, and the home of one of the ablest of the patriot war governors. Larry Gerlach also believes that New Jersey furnishes an excellent case history of how Americans moved from protest to rebellion and thence to secession from the empire. In this thoroughly researched and richly detailed study, he relates how the Revolution came to New Jersey between the years 1763 and 1776.

Gerlach views his subject largely from the vantage point of politics: his sources are more often the records of legislators and chief executives than the annals of the underside; and his book is admittedly "traditional narrative history." But in this genre, it is an outstanding product. What Gerlach's researches demonstrate is that New Jersey was an extremely unlikely candidate for rebellion in 1763. A land of ethnically diverse peoples, the province was nevertheless homogeneous enough socially and occupationally to avoid deep splits in its social structure. Its politics were diffuse and unstructured, but they spawned no great clashes of rival families or conflicts between differing political ideologies. The imperial measures which caused shock waves in other colonies produced only ripples in the garden colony.

In the revolutionary movement after 1763, New Jersey followed the lead of its neighbors rather than moving independently into rebellion. Always, Jerseyites showed a caution and moderation which made them more like their compatriots in Delaware and North Carolina than in Virginia or Massachusetts. New Jersey had irritants enough issuing from Whitehall—currency restrictions,



customs exactions, heavy troop quartering, and incompetent British-appointed judges—but they were felt more symbolically than substantively: they were continuing illustrations of New Jersey's subordination to British interests. In 1776 Jerseyites became reluctant rebels by the logic of events around them rather than from the pressures of domestic grievances. The revolution they made thereby was pre-eminently political and constitutional, not social or ideological. Thus, Gerlach concludes, New Jersey entered statehood as an act of secession from the empire, not as a protest against the social order.

Gerlach's documentation proves his case adequately enough, but is the documentation sufficiently comprehensive? Gerlach finds few ideological, philosophical, or even religious issues raised in the literature emanating from New Jersey. But if Jerseyites were so readily influenced by neighbors in their constitutional and political views, it seems hardly likely that they could have missed the psychological and ideological ferment reflected in the press of the other colonies. Then, too, Gerlach describes the movement toward independence in New Jersey as a rather broadly based steady progression, giving it a kind of inevitability which seems belied by the very active loyalism that developed in the state after 1776. Nevertheless, Gerlach makes a forceful case for his proposition that "New Jersey offers a classic example of the moderate rebellion launched to further liberty not license, designed to create not destroy, intended to reform not revolutionize." Whether New Jersey is thereby representative of the larger revolutionary movement or unique within it is a question that Gerlach's specialized account cannot really answer.

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HERBERT LEVENTHAL. *In the Shadow of the Enlightenment: Occultism and Renaissance Science in Eighteenth-Century America*. New York: New York University Press, 1976. Pp. 330. \$15.00.

Taking his cue from Paul Oskar Kristeller's dictum that "historians, like journalists, are apt to concentrate on news and to forget that there is a complex and broad situation which remained unaffected by the events of the moment," Herbert Leventhal has put together a book on the survival of Renaissance beliefs concerning the occult and of Renaissance science into the period of the American Enlightenment. In the process he has hunted down and assembled much curious and interesting information. His subject is cultural lag—sometimes related to class, as in the case of judicial astrology, which was generally ridiculed by edu-

cated persons in the eighteenth century yet survived in the popular almanacs, but more often not so related, as with natural astrology, which was generally accepted by all classes alike.

The subject is not, however, always clearly conceived. One wonders, for example, whether the best and most provocative chapter, on rattlesnake fascination, belongs in the book at all. Beliefs about rattlesnake fascination, although they eventually came to be associated with older beliefs about the evil eye, were not a cultural relic of the Renaissance but, as Leventhal himself demonstrates, a product of the Enlightenment.

There is an abundance of small errors, both typographical and grammatical, and some of them are to be found in quotations as well as in the author's own prose. More serious are the errors of fact and of interpretation. We are told, for example, that "burning in colonial America seems to have been reserved for the one offense considered even more heinous than witchcraft—slave revolts." But, in fact, burning was a consequence of established law, not of public opinion in colonial America. In England and in most English colonies it was the penalty for petty treason, and the murder of a master by a slave was one form of petty treason. Also we find the author constructing, on extremely inadequate evidence, a theory that the magic of the Pennsylvania Germans was "learned and sophisticated" as opposed to the folk magic of the English colonies. "Simple witches in the rest of the colonies," he tells us, "were never thought to have owned . . . a learned book." In the first place it is demonstrably untrue to assert that handbooks of magic, whether learned or unlearned, were not to be found in English America. In the second place, one wonders how scholarly the *grimoires* in use among eighteenth-century Pennsylvania Germans actually were. Most of them in use today are compendia of simple folk charms and recipes.

At times one has to question the author's sense of proportion, as when he relates a small hoax of Benjamin Franklin's, involving a reference to the Virgin Mary and then solemnly assures us that "although clever, this was perhaps not in the best of taste." And one occasionally finds him making a statement which simply does not make sense; he tells us, for example, that Charles Morton listed in his *Compendium Physicae* "five categories of spirits, exclusive of God," the last of which is "matter . . . with nothing of spirit about it." In short, while this book should help to open up a neglected area to investigation, much more work will be necessary before we begin to approach an adequate sense of it.

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MILTON W. HAMILTON. *Sir William Johnson: Colonial American, 1715-1763*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1976. Pp. xiv, 402. \$17.50.

In 1776 one observer remembered the late Sir William Johnson as Hamlet recalled his father: "He was a man, take him for all in all, I shall not look upon his like again." Perhaps it is this quality in him that makes his spirit so difficult to capture. The most recent attempt is by Milton W. Hamilton, coeditor of the Johnson Papers and former state historian of New York. The focus is on William Johnson's financial activities and his exploits during the French and Indian War. Of particular concern were Johnson's accomplishments vis-à-vis the English and New England officers serving with him.

Unfortunately, this is not the "fresh, critical" biography which the author had been moved to prepare out of his conviction that Johnson's "true historic importance . . . has yet to be fully appreciated." Too concerned with correcting the romanticized older studies, the biographer never reveals William Johnson the man and his motives. The questions, therefore, still remain to be answered. Why did the well-connected and able Johnson fail to ally himself by marriage to a powerful New York family? Why instead did he enter a number of informal liaisons? What was the darker side of this complex man? Why did he remain aloof from much of New York politics?

There is disappointment also in the description of William Johnson as Indian superintendent, for the native Americans are almost nonexistent in the book. Yet when an individual serves so long among the native peoples as did Johnson, readers of his biography are curious about his perceptions of the tribal peoples and theirs of him. Apparently there was no attempt in this study to incorporate pertinent anthropological literature on the Iroquois or recent scholarship in native American history.

Some of the gaps which appear in the scholarship would have been filled, no doubt, by the inclusion of a bibliography, but there is none. Wading through the notes, moreover, the reader will find a noticeable hiatus in recent references—not only in regard to the secondary literature, but also to such specific studies as David McKeith's dissertation on William Johnson and New York politics (Syracuse, 1971).

However much the publisher would like to promote this as the "definitive" biography of Sir William Johnson, it will have to be supplemented by the earlier, more popular treatments such as those by Pound and Day or Flexner, as well as by recent specialized studies.

JAMES H. O'DONNELL III  
Marietta College

LESTER J. CAPPON *et al.*, editors. *Atlas of Early American History: The Revolutionary Era, 1760-1790*. Princeton: Princeton University Press, for the Newberry Library and the Institute of Early American History and Culture. 1976. Pp. xvi, 157. \$125.00.

All persons familiar with the course of scholarship in early American history during recent decades are aware of the handsome contributions of Lester Cappon and Lawrence Towner. With this atlas they have almost outdone themselves. Cappon was the general editor, and Towner was primarily responsible for securing the necessary financial support. The Institute of Early American History and the Newberry Library were joint sponsors.

The planning of the atlas began about fifteen years ago, and the scheme of it was carefully and maturely framed. Although the maps and charts cover the Revolutionary period, they throw light upon the entire range of early American history. The approach is that of the historian rather than that of the historical geographer. The contents of the atlas are arranged in three divisions dealing with the colonies before 1775, the War of Independence, and the formative years ending with the census of 1790.

The scope of the information supplied in graphic form is remarkable and cannot be fully indicated in this review. It includes such matters as the locations of Indian villages, royal mail routes, residences of American members of learned societies, circulation of newspapers, locations of ironworks, locations of churches and academies, British customs districts, the spread of the news of the battle of Lexington-Concord, military activities and settlements of the Loyalists, Western settlements and territories, representation in Congress and state legislatures, and sentiment regarding the Constitution of 1787. An unusual and appealing feature is partial description of the War of Independence by means of maps that indicate the level of military activity in specific areas at specific times. Of course, the atlas contains maps illustrating the Proclamation of October 7, 1763, the treaty of peace of 1783, and territorial boundaries of all sorts. Especially notable in the opinion of this reviewer are the fine maps of the colonies and of colonial cities.

In sum, although the general editor refers to the atlas as complementary to that of Paullin and Wright, it amply covers the founding time of the American republic and does not need support from or reference to any other work. Indeed, it is superb.

JOHN R. ALDEN  
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RICHARD M. JELLISON, editor. *Society, Freedom, and Conscience: The American Revolution in Virginia, Mas-*

*sachusetts, and New York.* New York: W. W. Norton and Company. 1976. Pp. 233. \$9.95.

This slim yet weighty volume consists of revisions of the 1973-75 McClellan Lectures presented at Miami University (Ohio) in commemoration of the bicentennial of the American Revolution. Publication of these three extended essays treating "a significant aspect of the revolutionary years" in Virginia, Massachusetts, and New York is justified on the grounds that they collectively "offer not only important insights into the Revolution but also reveal the nature and thrust of current research in American colonial history."

In analyzing the political culture of eighteenth-century Virginia, Jack P. Greene continues his long-term study of the relationship between society, ideology, and politics in early America. His focus here is the Virginia gentry, that broad coterie based on wealth which composed 2 to 5 percent of the population, with special attention given the tightly knit social group of forty-odd families which formed the "inner gentry." Through a system of deference politics that placed a premium on men of demonstrable fortune and ability, a political code that demanded personal integrity and talent, and a social ideal that turned on stewardship and public service (a kind of political noblesse oblige), the gentry came to dominate all levels of government in the province. And that hegemony by an elite concerned more with practical politics than political philosophy produced by mid-century unprecedented harmony and tranquillity in public life. It was the threat to the status quo, Greene argues, posed by the fear of increased interference from imperial authorities abroad coupled with mounting socioeconomic regression at home that influenced the role of the Old Dominion in the coming of the Revolution.

In contrast to Greene's skillful synthesis of extant scholarship augmented by perceptive interpretations of his own, Richard Bushman plows virgin soil in probing the political consciousness of Massachusetts' farmers on the eve of the Revolution. Using the responses of the towns (featuring the heretofore overlooked reply from Hubbards-ton) to the famous "Boston Pamphlet" of 1772 as a frame of reference, Bushman discovers a greater degree of political awareness and sophistication among the rustics than previously believed. In fact, he contends there existed in the backcountry a "vernacular sociology" that transformed the rhetoric of rebellion into a well-known scenario. Fundamental to the agrarians' conceptualization of the Anglo-American crisis, he argues, was a specter of feudal oppression and descent into tenantry traceable in the Bay Colony to the protests of John Wise against Edmund Andros and the Dominion of New England. Thus on the eve of the Revolution contemporary debates over bishops and soldiers

awakened deep-seated hostility to lordship, both temporal and spiritual; abstract rhetoric such as "slavery" took on tangible reality in light of current examples of political corruption, burdensome taxation, and social stratification.

Where Greene and Bushman deepen our understanding of why some Americans rebelled, Michael Kammen reminds us that for many people the protest-independence movement was not so much a struggle for either home rule or rule at home as a tortuous crisis of conscience. Using New York as a case study, he portrays the plight of persons who hesitated to side with the congress or the crown, discusses the various ways in which the state attempted to constrain conscience and secure allegiance, and examines the ritual, rhetoric, and implications of oath-taking. Kammen is surely correct that the "reluctant partisans" are among the most neglected members of the Revolutionary generation. As this sensitive and insightful excursus makes clear, a better understanding of their position is essential if we are to appreciate fully the actions not only of the committed rebels and royalists but also of those charged with maintaining civil authority amid civil war and revolution.

Like all such publications, this compendium is vulnerable to easy criticism. Despite an excellent introduction by Richard Jellison, the disparate pieces do not form an integrated whole. Moreover, the lectures-turned-articles are by nature more exploratory than definitive. Yet to be demonstrated are, among other things, the specific ways in which British policies and provincial dislocations threatened the Virginia elite, the depth and pervasiveness of the feudal image in rural Massachusetts, and the wellsprings of the crises of allegiance which plagued New Yorkers. Then, too, the collection perpetuates the tyranny of the Big Three over Revolutionary literature: would that such neglected colonies as Delaware and North Carolina soon receive due consideration.

The foregoing comments are not, of course, negative. The volume deserves unqualified praise. Perhaps its component parts are best read with historiographical bifocals: as close examinations of important facets of the Revolution in three colonies and as suggestive explorations of themes with important implications for Revolutionary America as a whole.

LARRY R. GERLACH  
*University of Utah*

HELEN HILL MILLER. *George Mason: Gentleman Revolutionary.* Chapel Hill: University of North Carolina Press. 1975. Pp. xi. 388. \$18.95.

PAMELA C. COPELAND and RICHARD K. MACMASTER. *The Five George Masons: Patriots and Planters of Virginia and Maryland.* Charlottesville: University

Press of Virginia, for the Board of Regents of Gunston Hall. 1975. Pp. xiii, 341. \$17.50.

If few bicentennial tributes properly acknowledge the contributions of George Mason of Virginia, the authors of two admirable new books are not to blame. Helen Hill Miller and joint authors Pamela C. Copeland and Richard K. MacMaster in two quite different yet complementary biographies present persuasive briefs for extending greater historical attention to the squire of Gunston Hall. This new outburst of Mason scholarship relies heavily upon Robert Rutland's excellent three-volume edition of *The Papers of George Mason, 1725-1792* (1971), the starting point for all future students of Mason.

James Madison once remarked that Mason possessed the greatest talent for debate of anyone in his generation. Mason indeed employed his voice and his pen frequently and most effectively in the important political debates of the Revolutionary era. He was principal author of the Fairfax Resolves in 1774, father of the Virginia Declaration of Rights in 1776, and an important participant in the framing of Virginia's first written constitution that same year. In 1787 Mason journeyed to Philadelphia, his only trip outside the Chesapeake area, and contributed significantly to discussions at the constitutional convention. Rejecting finally the document he had helped to forge, Mason returned to Virginia to lead the battle there against ratification and to champion a federal bill of rights. He repeatedly used his considerable talents in public duty, although he was no doubt happiest when at home overseeing his beloved plantation and his family of nine children. Mason was a prototypical southern gentleman of the Enlightenment—concerned citizen, advocate of reform, sponsor of internal improvements, promoter of western lands, and patron of the arts. His biographers have an unenviable task.

Miller returns to her subject some thirty-seven years after she wrote *George Mason, Constitutionalist*. Her focus remains primarily on the public man and his political ideas, especially his contributions to the Bill of Rights and constitution-making, two subjects which have often commanded her scholarly and literary attention. This latest book is less a traditional biography than a sweeping story of the colonial move toward independence, the extension of liberty in America, and the establishment of a new republican government. In such a grand drama, Miller often relegates Mason to the stage wings, and the scene of action is as likely to be Philadelphia, Boston, or even France, as it is the Northern Neck of Virginia, which Mason seldom left.

Miller's *George Mason: Gentleman Revolutionary* is elegantly written and profusely illustrated. The

104 accompanying plates underline the broader scope of this work. Most are portraits of Mason's contemporaries or illustrations of documents, seals, buildings and drawings that are only loosely associated with Mason. While they aid in making this an exceptionally handsome book, one wonders if the cost could not have been reduced substantially with the omission of half or more of these peripheral plates. Finally, Miller has chosen to rely almost exclusively on primary materials which she has combed assiduously and quotes extensively. Greater analysis and a judicious use of the vast secondary literature would have made this a richer and less simplistic work.

Copeland and MacMaster, in contrast, have largely eschewed the national arena in their genealogical history of *The Five George Masons*, but the result is by no means a parochial study. While devoting at least half of their book to George Mason IV, the patriot, they still present a model of a thoughtful multi-generational family history. The authors portray in some detail successive Masons, from the immigrant who arrived in Virginia in 1651 or 1652 through the sons of the "revolutionary gentleman," and exercise care to place each man in his own social, economic, religious, and political milieu. The result is a skillful interweaving of biographies with a suggestive history of the region between the Rappahannock and Potomac Rivers. Copeland and MacMaster trace the development of the plantation system, the feeble growth of local institutions, and the intermittent intrusion into the Northern Neck of larger issues from the outside world, all seen primarily through the Masons. Especially welcome is their presentation of a definite community spanning the Potomac River to link Virginia's Northern Neck with part of Maryland's lower Western Shore. These regions had more in common with each other than either did with the remaining areas of their respective colonies. This was Mason's world; he was pre-eminently a local man, which the authors convey successfully without shortchanging his role in provincial and continental affairs. They conclude the book with his sons who spent most of their adult lives outside Virginia and thus reflect the end of an era.

Each of these books can well stand on its own. Where they overlap, they are in remarkable agreement; where they diverge, each nicely counterbalances the other's emphasis and reinforces its strengths.

DAVID W. JORDAN  
Grinnell College

RENÉ DE LA CROIX, DUC DE CASTRIES. *La France et l'Indépendance américaine: Le livre du bicentenaire de l'Indépendance*. Foreword by DUC DE LÉVIS MIREPOIX. Paris: Librairie Académique Perrin. 1975. Pp. 380. 50 fr.



BUCHANAN PARKER THOMSON. *Spain: Forgotten Ally of the American Revolution*. North Quincy, Mass.: Christopher Publishing House. 1976. Pp. 250. \$9.75.

These books were written in commemoration of the bicentennial of the American Revolution. The Duc de Castries presents a graceful summary of the American Revolution from the French perspective. Buchanan Parker Thomson describes Spanish aid to the United States; although Spanish assistance was small in comparison to French aid, Thomson argues that it was important. Neither author is analytical in his treatment of the subject; neither incorporates scholarly work in the field, choosing instead to emphasize personalities. Castries accords credit for almost every development to Benjamin Franklin and George Washington. Thomson stresses the neglected role of Don Diego de Gardoqui in Spain and the more familiar actions of Bernardo de Galvez in the Mississippi Valley, without showing how either man was instrumental in carrying out Spanish policy.

Castries combines both military and diplomatic aspects of the Revolutionary War as seen from Paris. His portraits of French leaders are informed but not original. He finds the Count de Vergennes less than a genius, but nevertheless successful in honorably maintaining the conflicting alliances with Spain and the United States. General Rochambeau receives high marks, while Admirals D'Estaing and De Grasse suffer criticism for their unavoidable defeats at the hands of the British. The author glides lightly over the American side of the war except for the battle of Yorktown. Irritating factual errors about the United States and its leaders suggest that he is not fully informed about this part of his subject.

The subtitle of Thomson's book indicates its major thesis. Although the author repeatedly declares that her information is new, most works dealing with European diplomacy tell the same story. Thomson cites the Spanish archives extensively but not always to best advantage. In the first part of the book long, unnecessary documents, such as cargo manifests, are reprinted; marginalia not written by the author of a document are reproduced as if they were part of the original text. In the second part of the book the author discusses Spanish-American attacks in the Mississippi Valley, but she adds little, if anything, to previous accounts.

Both authors single out John Jay for special opprobrium. Thomson attributes Jay's failures in Spain to his wife's anger at not being received by the Spanish Court. Castries unfairly describes Jay's conduct in the peace negotiations with Great Britain, when he departed from the French Alliance, as behavior comparable to Benedict Ar-

nold's. Clearly what is needed is not a new appraisal of John Jay, but a systematic analysis of Spain during the American Revolution. In bits and pieces both authors give hints that the full story of Spanish-French and Spanish-American relations is not known. Despite the appearance of these two books, historians will still have to rely on the works of Samuel F. Bemis and Jonathan Dull to understand European participation in the American Revolution.

WILLIAM STINCHCOMBE  
Syracuse University

R. ARTHUR BOWLER. *Logistics and the Failure of the British Army in America, 1775-1783*. Princeton: Princeton University Press. 1975. Pp. xi, 290. \$12.50.

DAVE RICHARD PALMER. *The Way of the Fox: American Strategy in the War for America, 1775-1783*. (Contributions in Military History, number 8.) Westport, Conn.: Greenwood Press. 1975. Pp. xx, 229. \$12.50.

These two books add a great deal to our understanding of the Revolutionary War, but they do so in words that were unfamiliar in the eighteenth century. R. Arthur Bowler observes in his preface that the word "logistics" comes from the French *logistique* and did not achieve its comprehensive, modern meaning—including procurement, transportation, supply, and maintenance of personnel and equipment—until the Second World War. Dave Richard Palmer, likewise, admits that "'strategy' was not a word George Washington used." It came into the language in the form of "stratagem," and some years after Washington's death was used to refer to "movement beyond the visual circle of the enemy or out of canon shot." Karl von Clausewitz introduced the modern definition, distinguishing strategy from tactics, in analyzing the campaigns of Napoleon.

Both Bowler and Palmer insist, however, that the concepts were familiar to the Revolutionary generation—just as sea power existed before Mahan, and sex before Freud. Bowler explains in detail the predicament of a large regular army three thousand miles from its home base, and Palmer describes the advantages enjoyed by a national liberation force supported by powerful friends. Bowler's is the more specialized of the two books. It is the result of several years' work in England, Canada, and the United States, and is based primarily on British service records, especially those of the commissary, quartermaster, and barrackmaster generals in the Public Record Office and elsewhere; and there is an excellent fourteen-page bibliography to help scholars who may wish to retrace the author's steps.



The British faced unprecedented difficulties during the American Revolution. Their system of supply was based largely on private enterprise, with much use of local bankers and contractors—a system which had always worked well in earlier wars, but was bound to fail when the American population turned hostile. The consequence of this change was that the British forces found themselves cooped up in small areas protected by the armed forces, and most of their supplies had to be brought from “home.” Foraging parties needed protection by troops, and the skirmishes that ensued naturally caused increased hostility. The generals could, on occasion, get what they wanted by paying hard cash, but this was expensive.

Several campaigns, perhaps all, were affected by these difficulties. Howe’s reluctance to get moving in the spring—which was roundly condemned by the Loyalists—can be understood more clearly if his supply problems are taken into account. Clinton’s refusal to release supply ships for the return trip to England, though this policy retarded his logistical build-up, can also be understood when the implications of impending evacuation from Philadelphia, or even possibly from New York, are fully appreciated. The British forces in New York came close to suffering the fate of those in Yorktown on more than one occasion. Bowler provides, as an appendix, a chart depicting British Army food reserves from 1775 to 1781. Unfortunately for the British, their supply problems were never really solved until 1781, and by then it was too late. Meanwhile, many of the supply officers had quietly accumulated private fortunes.

Palmer’s thesis is that George Washington is not to be thought of merely as a Fabian general, who delayed and stalled until the British managed to lose the war. He was offensive and almost overly aggressive in 1775–76, defensive in 1776–78, aggressive again after the French alliance, defensive during the home-front collapse in 1779–80 (except for the Sullivan Expedition), and finally offensive with the help of the French in the Yorktown campaign of 1781. During all of these twists and turns, necessitated by events beyond his control, Washington kept firmly in mind the twin objectives of independence and geographical expansion. His response to a variety of challenges was both flexible and vigorous; he deserves as much credit for strategic subtlety as he does for character. The British, likewise, tried several strategies—the difference being that in most cases they failed. On their side only Guy Carleton emerged from the war with reputation enhanced.

Both books are well-written—though one may question Palmer’s reference to the Indians as “ferocious” (p. 88) “savages” (p. 87)—and both are useful additions to an undergraduate reading list. In fact, both make original and valuable contribu-

tions to the American people’s bicentennial discussions.

JOHN M. COLEMAN  
Lafayette College

JOHN SHY. *A People Numerous and Armed: Reflections on the Military Struggle for American Independence*. New York: Oxford University Press. 1976. Pp. xv, 304. Cloth \$12.95, paper \$3.95.

These ten essays and lectures were prepared by John Shy over a period of time for a variety of reasons. In fact, he suggests as a subtitle, “Accidental Essays on the American Revolution.” Shy quite frankly admits that he is striving to act as something of an evangelist for military history, who “would make students of history pay attention to a subject they are ignoring at their peril.” He correctly maintains that war changes society, that strategy and military policy are aspects of politics, that incidence of military service reflects and effects social structures, and that the events and patterns of armed struggle help shape the way people think about themselves and others. The author seems to feel that the efforts of the strictly military historian and the social historian (who generally ignores the war as of little use) should be melded together so as to show the cause and effect of social change. In his chapter entitled “The Military Conflict Considered as a Revolutionary War,” he sees the war “not as an instrument of policy or a sequence of military operations, but as a social process of political education that can be explored, and should be analyzed” (p. 224). Every war fought over control of a territory has to result in political, economic, and social changes.

My greatest disagreement would come with the essay, “A New Look at the Colonial Militia,” which could be considered as a defense of the military actions of that group. Shy states that despite the low opinion held of militia by their contemporaries, a community threatened by an enemy could be counted upon to come to its own defense. This was often the case, but I feel that a qualifying statement should have been added to suggest that this was usually true when opposing troops were of a similar type; when acting against, or sometimes with, more disciplined soldiers, their behavior was at times erratic. And one other point of criticism should be directed toward “The American Military Experience,” not so much as to content but for the shift in style. Here the author seems a bit unsure of himself and his prose almost plods along, much like an arthritic soldier attempting close order drill. Perhaps the excellence of the preceding essays spoils the reader.

Yet, despite criticisms (primarily of interpretations) every graduate student, and some profes-

sors, studying the American Revolution should be required to read this excellent collection of essays. In fact, I wish I had written them—even those I disagree with.

HUGH F. RANKIN  
Tulane University

ROBERT A. GROSS. *The Minutemen and Their World*. (American Century Series.) New York: Hill and Wang, 1976. Pp. xiii, 242. \$3.95.

This brief history of Concord, Massachusetts during the revolutionary era is superb. In the mode of recent studies of colonial New England communities, the book is a thorough description of local society and politics based on massive research in town and colony records, private papers, and genealogies. Yet, much to the author's credit, the tables, charts, and social science jargon that necessarily emerge from such research are banished into a multitude of footnotes mercifully put at the back of the book, thereby allowing for a deftly written text, readily understood and enjoyed by scholar and general reader alike.

Concord holds, of course, a key place in historical accounts of the outbreak of the Revolution and the author has a fine feel for the inherent drama of conflicting fears and aspirations in the community. Personalities and problems of people from all classes, including women and blacks, appear coherently in a changing social context and provide examples and symbols of the larger forces described in the study.

Finally, and most important for the scholar, the author, in his description of Concord, develops a challenging synthesis of recent interpretations of eighteenth-century New England. Concord experienced constant pressure of population on land. There were the usual communal hopes and ideals thwarted by sectionalism, demographic pressures, and religious quarrels. Robert Gross avers, in uniformity with recent scholarship, that revolutionary ideals were attractive to the townsmen as a means of regenerating communal élan rather than defending democratic individualism. At first élan returned. But the war and its aftermath transformed the town into ever more modern political and economic forms until the community of Emerson and Thoreau emerged, a place radically different from the Concord of 1760.

Anyone even vaguely interested in New England towns and the causes and consequences of the American Revolution would be well advised to read this book carefully, including even the footnotes.

RICHARD P. GILDRIE  
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RAOUL F. CAMUS. *Military Music of the American Revolution*. Chapel Hill: University of North Carolina Press, 1976. Pp. xii, 218.

*Military Music of the American Revolution* begins with a careful survey of eighteenth-century military music in Europe, including an account of its previous historical development. After a briefer overview of martial music and its cultural environment in the thirteen colonies on the eve of the Revolution, the author plunges into the chronology of the war itself, setting forth whatever evidence he has been able to unearth about the state of military music (mainly in the Continental army) for each stage of the conflict. He makes a distinction between two groups that made military music: the field music and the band of music. The field music (principally drummers and fifers) kept cadence and signaled the various actions that soldiers were to take on the march or in camp. The American command was concerned about improving the field music in the Continental army, and Raoul Camus imaginatively reconstructs the actual music that was probably intended for each of the beats or signals given in Baron von Steuben's *Regulations* of 1779. The band of music (principally oboes, clarinets, French horns, and bassoons) provided music for parades and other ceremonies and also entertained the officers at meals and parties. In Europe and Britain, the officers of a regiment customarily maintained a band at their own expense; but in the Continental army, few bands developed, and most of those that did seem to have had a rather fitful existence.

The author comes to his work not through the historical profession, but as a bandmaster and a scholar in the field of music. He has done extensive research on his subject and evidently speaks with authority about it. But most historians will find this book very tedious to read and too close to the doctoral dissertation from which it was drawn. Music—especially the music of the bands, to which so much of the book is devoted—was a very peripheral part of the American side of the Revolution. Camus has gathered a large number of surviving facts and contemporary comments, many of them of a casual nature, concerning the music of the Revolution, and he has packed them so closely together in his chapters that the result often seems more like antiquarianism than history. Many readers will not be able to see the forest for the trees.

FRANK R. ROSSITER  
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Dallas

RICHARD G. MILLER. *Philadelphia—The Federalist City: A Study of Urban Politics, 1789–1801*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press, 1976. Pp. xi, 192. \$12.95.

In this excellent case study of party development in America's largest city in the 1790s, Richard G. Miller argues that "local conditions" rather than congressional conflicts generated the Federalist and Republican parties. Utilizing census and tax records, Miller reconstructs Philadelphia's social structure to explore plausibly, if not conclusively, the relationship between party preferences and variations in occupation and wealth. Using simple correlations Miller suggests that Federalism is usually associated with more than average wealth and the occupations of high status. Other data, such as a collective biography of party leadership, reveal that Federalist elites were richer and drawn from socially more prestigious strata than the Republicans who extended greater recognition to artisans, men of middling wealth, and the less well-established ethnocultural groups. The demographic heterogeneity of Philadelphia's wards, however, limits the usefulness of the quantitative findings which are often, though not always, inconclusive, and often not adequately explained. For example, Miller does not satisfactorily show why there is a positive association between the woodworking trades and the Republicans but a negative one between the metal trades and the party. Fortunately, Miller buttresses his interpretations with literary data. He was unable to analyze quantitatively ethnic and religious variables, though here again scattered, impressionistic evidence suggests that they very much shaped the way people voted, with Germans, Irish, and Scotch-Irish Presbyterians usually leaning toward the Republicans, and Quakers shifting to that column in the late 1790s. Miller might have studied more deeply the role of denominational and ethnic attachments, for example, by identifying the location of churches by ward and exploiting church membership data and published sermons.

Above all, Miller has made a valuable contribution to the ongoing study of the process of political socialization in eighteenth-century America. First, he traced carefully the slow, tortuous, and haphazard development of party organization and popular participation. Second, he demonstrates, as others have for other times and places, that rivalry among factions within the city's upper strata was one of the most important factors in promoting party development. The Republican party, he argues convincingly, grew out of the efforts of lesser gentry to challenge the exclusivity of the "top dogs" by mobilizing artisans, working folk, and other politically apathetic elements. Between 1790 and 1800 voter turnout more than doubled as an increasing number of people came to perceive government instrumentally. Finally, Miller skillfully shows how the interplay of national issues with local circumstances and interests enormously increased the intensity of political life.

"The enemies of the rich are the enemies of the poor," Federalists warned the artisans and shopkeepers. "Elect to office the enemies of property and public confidence is shaken to its foundation." By the late 1790s such appeals fell on deaf ears, for, as Miller illustrates in this concise study, the erosion of habits of deference slowly but inexorably altered the foundations upon which "public confidence" would henceforth rest.

PAUL GOODMAN  
University of California,  
Davis

WILLIAM H. WILLIAMS. *America's First Hospital: The Pennsylvania Hospital, 1751-1841*. Wayne, Pa.: Havford House. 1976. Pp. 186. \$12.50.

This scholarly account of Anglo-America's first hospital covers the period from its founding in 1751 to the early nineteenth century, when "medical conservatism" set in and leadership passed to other institutions. The focus of the book is on hospital administration, not patient care. Williams relies heavily on the minutes of the board of managers and thus tends to see events from a boardroom perspective. His documentation is good, though he is not always discriminating; over two pages, for example, are devoted to the managers' efforts to obtain a Benjamin West painting.

Only two chapters deal primarily with the hospital's healing activities, and here the coverage is frustratingly spotty. Williams tells us that the hospital accepted both paying patients and the "worthy poor"; that charity patients outnumbered paying ones until the 1780s; that the mentally ill, often a majority after the 1780s, frequently came from the middle and upper classes; that from 1767 to 1769 the percentage of in-patients with venereal diseases rose from 9% to 11%; that surgical operations increased during the 19th century; that 38% of the in-patients in 1840 were Irish; and that sailors comprised the largest occupational group in 1840. But beyond these scattered facts we learn virtually nothing about the patients' social backgrounds and medical problems. Since the physicians' records for the eighteenth century are lost, Williams can be forgiven for not discussing therapy in that period, but we deserve to know more about the nineteenth century than that "bleeding was universally prescribed."

Particularly significant is Williams' discovery that the Pennsylvania Hospital was not a "gateway to death," as hospitals of the time have often been described. Except for the Revolutionary War period, mortality rates at the hospital never rose above 13%, partially because it customarily admitted only the potentially curable. There is no evidence, however, that the Pennsylvania Hospital was safer than those in England.

Despite its limitations, this book provides American social and medical historians with welcome information about one of the most neglected institutions in the field, the American hospital. For this Williams deserves credit.

RONALD L. NUMBERS  
University of Wisconsin,  
Madison

JOHN L. KESSELL. *Friars, Soldiers, and Reformers: Hispanic Arizona and the Sonora Mission Frontier, 1767-1856*. Tucson: University of Arizona Press. 1976. Pp. xv, 347. Cloth \$14.50, paper \$8.35.

In recent decades bits and pieces of the post-1767 story of the Sonora-Arizona frontier have been accumulating—biographical sketches, studies on policies and institutions, events and places, translations and/or editions of documentary sources. This mounting mass of materials has needed sorting, evaluation, supplementing, and synthesizing. The author has addressed that challenge and answered it well, interestingly, enlighteningly, and in a most readable fashion, as is his wont.

He traces the story of the Franciscan takeover following the expulsion of the Jesuits, the work of the gray robes of the Querétaro college in Pimería Alta and the blue robes of the Jalisco college in Pimería Baja. He tells of their fight with the Enlightenment-inspired "reformers," such as José de Gálvez, over the missionary approach. He also describes their conflict with Sonora's first bishop, Antonio de los Reyes—the traditional relationship of missionary to Indian ultimately won out. He notes the impact of the military on that far frontier. He relates, with understanding, the less edifying aspects of Franciscan frontier life, the often bitter quarrels of the friars among themselves and sometimes their moral lapses. He details the decline of the missions through the Mexican years of inefficiency, defensive ineffectiveness, and often ill-concealed anticlericalism. Arizona was near spiritual bankruptcy and dangerously Apache-ridden when the United States inherited the territory by the Treaty of Guadalupe-Hidalgo and the Gadsden Purchase of 1854; even the Franciscans were gone, expelled in the late 1820s, as the Jesuits had been a half-century before.

The volume has several dozen pertinent illustrations, several maps, and an extensive bibliography. Each chapter is headed with a helpful table of "Concurrent Events." The study is a most welcome contribution to Borderlands historiography.

JOHN FRANCIS BANNON  
St. Louis University

ABRAHAM P. NASATIR. *Borderland in Retreat: From Spanish Louisiana to the Far Southwest*. Albuquerque: University of New Mexico Press. 1976. Pp. x, 175. \$12.00.

In seven brief essays Abraham P. Nasatir sets out to "synthesize my life's work." The result is an ambitious and useful summary of the ways in which Spain tried and failed to defend Louisiana in the late eighteenth and early nineteenth centuries.

Nasatir sees Spain's slow retreat in Louisiana as a process whereby France, England, and/or the United States competed for six "frontiers" in the vast region between the Mississippi and the Rockies. Nasatir defines these frontiers as the lower Mississippi; the upper Mississippi; the Missouri; the Louisiana-Texas border; the area crossed by the Santa Fe trail; and the New Mexico-Louisiana border after 1803. This division of Spanish Louisiana into six subregions is a useful organizing device. Nasatir devotes a separate chapter to each frontier. His decision to make those chapters complete in themselves leads to considerable repetition, however, and his focus on the parts tends to obscure the whole.

In describing the lower Mississippi and the Texas-Louisiana frontiers, Nasatir relies heavily upon the work of other scholars. He bases much of the remainder of this synthesis, however, on his own monumental archival research spanning over half a century. Nasatir's effort at synthesizing his own work is especially welcome since his writing has usually taken the form of lengthy, detailed studies such as *Spanish War Vessels on the Mississippi, 1792-1796* (1968), *Pedro Vial and the Roads to Santa Fe* (1967), and the two-volume collection of documents relating to the Missouri, *Before Lewis and Clark . . . 1785-1804* (1952). In addition to summarizing these, Nasatir presents fresh information from his own unpublished work as well. He avoids footnotes, but indicates his principal sources at the end of each chapter.

Curiously, this overview may be of interest chiefly to specialists because the general reader, for whom it seems intended, is likely to be put off by a difficult and confusing writing style. Nasatir's meaning is often unclear; his identification of persons and places is inadequate (who, for example, were the "Cuampa" [p. 141] or "Cuampe" [p. 157] Indians?); his maps do not identify many place names mentioned in the text; and some of his generalizations are not supported by evidence. One wonders, for example, how Spanish explorers "paved the way" for Lewis and Clark (p. 84).

DAVID J. WEBER  
Southern Methodist University

JOHN LOGAN ALLEN. *Passage Through the Garden: Lewis and Clark and the Image of the American Northwest*. Urbana: University of Illinois Press. 1975. Pp. xxvi, 412. \$18.50.

The questions posed by geographer John Logan Allen are these: How much knowledge of the



Northwest did Lewis and Clark possess prior to their expedition? What were their sources of knowledge? What did they learn on their journey? How did their perceptions change as a result of the expedition?

Pursuing his research with a thoroughness that commands the respect of the most diligent historian, Allen has identified every map, narrative, and rumor that must have been known to Lewis and Clark. He has drawn a profile of the Northwest as it was then known. And he has analyzed the observations of Lewis and Clark on their expedition and speculated on how they changed their views. In a sense he has taken known quantities of data and fed them into human computers—the minds of Messrs. Lewis and Clark—and has then analyzed the subsequent printouts. The result is a major contribution to the Lewis and Clark record. *Passage Through the Garden* is a valuable study.

Among the intriguing facts brought out by Allen is the impression among Europeans from the time of the discovery of the New World that there must be straits, a lake, or a short highlands region containing a water divide linking the Atlantic and Pacific oceans. Still another is the pyramidal height-of-land concept, with radical drainage patterns. Somewhere in the far Northwest this height-of-land was supposed to exist, the source of such great rivers as the Missouri, Rio Grande, Colorado, Columbia, and others even less known.

It is also clear that the explorers envisioned the Rockies as mountains similar to the Blue Ridge, and believed, in accord with the ideas of the time, that a narrow divide across the mountains separated the navigable waters of the Missouri from those of the river known as Columbia.

Of added interest are forty-seven illustrations, most of them reproductions of rare maps. One of them discovered by the author was almost certainly carried by Lewis and Clark on their expedition. All of the illustrations are carefully placed in relation to their references in the text.

No one interested in Lewis and Clark or the status of geographical knowledge at the time of their expedition should be without this book. Some readers, including this reviewer, may not like the imprecise phrases "Garden of the World" and "Passage to India"—there are some valid criticisms to be made of them—but in spite of their wide use by Allen, this book is sound, interesting, and significant.

RICHARD A. BARTLETT  
*Florida State University*

JAMES PENICK, JR. *The New Madrid Earthquakes of 1811-1812*. Columbia: University of Missouri Press. 1976. Pp. 181. \$10.00.

During the early morning hours of Monday, December 16, 1811, a series of earth tremors began which were to have a dramatic effect upon the Mississippi Valley and to a lesser extent make themselves felt to other far corners of the United States. Over the next three months some 1,800 shocks would be felt, most of them minor, with two more—on January 23 and February 7, 1812—of an equally severe intensity with that of mid-December. Although no modern seismograph was available to measure them, these earthquakes have been generally assumed to be among the most severe in modern American history. They came to bear the name of a small town in southeastern Missouri—New Madrid—because of its devastation in the second major quake although the epicenter was some sixty-five miles to the southwest in northeastern Arkansas. The quakes caused widespread destruction and left the land considerably changed while the Mississippi River briefly "ran upstream." Those settlers with damaged lands were given the opportunity to exchange them elsewhere in the Missouri Territory by act of Congress—a practice which plagued land officials and territorial records for decades to come.

James Penick has put together a well-written account of the quakes and their effects upon people, animals, waterways, and land. Based on the scattered accounts of the times it offers a good insight into the reactions of persons suddenly confronted with the perils of the unknown. The vivid description of the devastation wrought upon the face of the land gives a picture of dramatic change brought about by the upheaval of natural forces. In short, in reading Penick's work one is readily caught up in the total violence of the event.

The book is attractively designed in an elongated style which places the footnotes to the side rather than at the bottom of the page.

WILLIAM E. PARRISH  
*Westminster College*

ROBERT P. SWIERENGA. *Acres for Cents: Delinquent Tax Auctions in Frontier Iowa*. (Contributions in American History, number 46.) Westport, Conn.: Greenwood Press. 1976. Pp. xix, 262. \$14.95.

Robert P. Swierenga's *Acres For Cents: Delinquent Tax Auctions in Frontier Iowa* is a by-product of his earlier *Pioneers and Profits: Land Speculation on the Iowa Frontier*. Through a combination of quantitative and conventional—or impressionistic, as he likes to call it—research he concluded that investments in Iowa lands in the fifties were almost certain to be profitable, but that "non-resident investors [note they are not called speculators] served a useful and often beneficial function." Despite such impressionistic judgment that work is a



landmark in the application of quantitative methods to the study of the West's land business and, incidentally, of the cost of speculator intrusion to actual settlers.

The same intensive application of quantitative methods and of research in documents has enabled the author to produce another useful though somewhat limited study of litigation over tax titles, of Iowa's legislative efforts to make them acceptable to the courts, of the elements that participated in the litigation, and of the profits derived from it. He centers his quantitative work on sixteen counties which seem to be fairly representative of the state, and, as in his previous study, finds much of use in the investment experiences of James S. Easley.

Frontier newspapers made frequent references to controversies over tax titles and to the problems land speculators fell into when their taxes were not paid on time. Sometimes, it was charged, speculators were delinquent in tax payments because assessment notices were deliberately published too late to allow them time to meet the deadline. The subject has not been neglected, but no one has pursued its legal ramifications or touched upon the new angle that Swierenga has used here; tax title auctions were "a money lending mechanism for local farmers, a government managed loan agency." Farmers found it was cheaper to let their taxes become delinquent and to pay the heavy interest and fees the law required because all the expenses involved were less than it would have cost them to pay their taxes at current interest. Investments in tax titles by outsiders also considerably increased the capital available in money-starved communities.

Swierenga is not much interested in the farmer whose taxes became delinquent and whose title had a lien against it. In Iowa for the 1850s and beyond he could conclude that the tax titles were not an index of agrarian problems other than the perennial one of inadequate credit. One might ask whether the Iowa Occupancy Law of 1851, in combination with the color of title acquired by a tax title would benefit a squatter or be more useful to the owner of the fee. Some comment as to the significance of the publication of the tax title list to the financial health of local newspapers might have been included.

PAUL W. GATES  
Cornell University

JAY P. DOLAN. *The Immigrant Church: New York's Irish and German Catholics, 1815-1865*. Foreword by MARTIN E. MARTY. Baltimore: Johns Hopkins University Press. 1975. Pp. xiv, 221. \$10.00.

Jay P. Dolan's *The Immigrant Church* is an important contribution to American urban, ethnic, and

Catholic history. Interested in the "man in the pew" rather than in diocesan administrators, Dolan focuses on the nationality (in reality, language) parishes serving Irish and German immigrants in New York City between 1815 and 1865. In such parishes as Transfiguration (Irish) and Most Holy Redeemer (German), which the author examines in considerable detail, the newcomers practiced their faith in the familiar ways brought from their homeland. Despite differences between Irish and German practices (the latter, for example, especially emphasized music and elaborate ceremonial) and frequent tension between the two nationalities (such as in the ethnically mixed St. Alphonsus' Church), the unity of the Church was maintained, in part because of a common Tridentine faith, Protestant nativism which limited the centrifugal forces of ethnicity and increasing centralization of authority or, as Dolan terms it, "boss rule in the Church" under the Archbishop, in this case John Hughes.

Although there is little startlingly new in Dolan's general assessment of the role of the nationality parish, several of his conclusions broaden significantly our understanding of Catholic ethnicity in New York and, by implication, in other major immigrant receiving cities. Religious indifference, for example, was widespread and attendance at Mass low, especially among the Irish, not simply because of insufficient churches and priests but also because of religious ignorance. Consequently, the Church had to help immigrants who already had faith to maintain it and also stimulate slackers to become practicing Catholics. Furthermore, the parish was not coterminous with the ethnic community as a whole. Nor did the parish fit the traditional picture of stability and homogeneity. Residential mobility was high, and significant economic differences distinguished parish membership. Chapters on parish charities, preaching, parochial schools, and missions (which approximated Protestant revivals) are equally revealing.

Dolan's portrayal of the ordinary immigrant Catholic is occasionally quite dry, and his actors, including Hughes, are consequently rather lifeless at times. One also looks in vain, and perhaps unreasonably, for more information on the large number of nominal Catholics. These are merely quibbles, however, for Dolan has produced an excellent book which all historians of American Catholicism and immigrant life will read with profit.

HENRY B. LEONARD  
Kent State University

STUART M. BLUMIN. *The Urban Threshold: Growth and Change in a Nineteenth-Century American Community*.

Chicago: University of Chicago Press. 1976. Pp. xiv, 298. \$16.50.

In his study of Springfield, Massachusetts Michael Frisch explored the transition from town to city. In *The Urban Threshold* Stuart M. Blumin searches for another of the "missing links" in the historical process of urbanization, that of the change from rural village to city. Among the questions he asks is whether or not the approach of the "urban threshold" by a rural community necessarily involves "the fragmentation of a closely knit community, the loss of intimate relationships, and the erosion of the autonomy and self-sufficiency of the community as a whole." His answer is that contrary to the experiences of the rapidly growing cities of the Midwest, those older rural communities that grew into small cities as a result of national and regional changes, such as the industrial and transportation revolutions, developed a stronger sense of community identity and action.

The community Blumin analyzes is Kingston, New York, a loose collection of rural villages until 1826 when it was transformed by the decision of the Delaware and Hudson Canal Company to connect the coal fields of Pennsylvania with the Hudson River. He uses local newspapers, town and church records, diaries of inhabitants, and a good measure of quantitative data to cover Kingston's transition from an individualistic rural society to a small urban community. The population and physical changes are clearly defined as are the development of formal political and social institutions. His chapter on participation in the urban society will serve as a model both for its methodology and for its conclusions.

*The Urban Threshold* is a significant book because it calls attention to the urbanization of a truly rural community. Perhaps as important, however, is the fact Blumin has been able to combine sophisticated social science techniques and traditional methods in a comfortable style that suggests the old argument between the "quantifiers" and the "humanists" is becoming passé.

If Blumin's book has a weakness, it is the selection of Kingston. Instead of a single community, Kingston includes two "villages"—Kingston, the older rural village, and Rondout, the newer settlement that developed in response to the canal's construction. Blumin is cautious about making broad claims of Kingston's representativeness, but one wishes he had tested his ideas in a case study not so idiosyncratic. His definitions of "town" or rural community do not apply well to areas outside the Northeast. As a result further studies will be needed to confirm Blumin's conclusions on community and urban growth.

DEAN R. ESSLINGER  
Towson State University

KATHLEEN NEILS CONZEN. *Immigrant Milwaukee, 1836-1860: Accommodation and Community in a Frontier City.* (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1976. Pp. 300. \$16.50.

Not all the immigrants bowed 'neath the blows of culture-shock: "*Ach, Heer Jeses,*" said a Milwaukee woman in 1860, "*so scheene is es doch nergends wie in Milwaukee!*" Kathleen Neils Conzen's *Immigrant Milwaukee* rests upon an impressive statistical picture of community development during the period and upon contemporary literary sources which illustrate individual experiences of that development. The introduction highlights conceptualizations regarding ethnicity and acculturation which need testing through the reconstruction of historical processes, and which also offer directions and questions for historians confronting a bewildering variety of data. The book deals mostly with the Germans, less with the Irish, and very little with other national groups. This reflects the relative impacts these groups had on antebellum Milwaukee and also allows the author to concentrate on the richest body of material.

Milwaukee Germans soon were a large enough group to moderate their cultural difficulties by developing a community of their own. The German community became so large and diversified by 1860, in fact, that it began to lose its group identity as class, religious, and other tensions arose within it. Comparison with Milwaukee's Irish population, less heterogeneous and producing less in the way of ethnic institutional life, is highly interesting. Slum life began early for most of the Irish; frame buildings no longer needed in the business center were towed into the "Bloody Third," where overcrowding made them profitable. "Yankee ingenuity thus provided the new city with substandard housing for conversion to immigrant tenements."

Economic growth, changing social structure and age-sex ratios, religious and occupational differentiation, and the development of neighborhoods are carefully described, and make a three-dimensional background for a splendid, and culminating, chapter on Milwaukee's early ethnic politics. Generalizing on the urban or rural old-country upbringing of immigrants is still educated guesswork, including Conzen's. Also, land ownership as an index for economic class quite likely leads us to overstate the concentration of wealth; its relation to class consciousness is even more obscure.

Rather than concentrate on such possibly dubious matters, however, one should emphasize the book's great merit. This resides primarily in its marriage of quantification and narrative. Quantification versus literary narrative is a shallow posing of issues. Some paragraphs of Conzen's book

will be hard going for readers untrained in statistics, but her writing is for the most part vigorous and clear, with a fine and dramatic sense for the individual representative or exception. *Immigrant Milwaukee* is an excellent example of method in research and writing.

A. THEODORE BROWN  
University of Wisconsin—  
Milwaukee

REX BURNS. *Success in America: The Yeoman Dream and the Industrial Revolution*. Amherst: University of Massachusetts Press. 1976. Pp. x, 212. \$10.00.

The theme of success in the American mind has long fascinated historians. In the past decade the number of books treating this subject has grown quite large. Rex Burns' *Success in America* is the most recent addition to that list.

Burns feels the need to justify yet another work in a field that has been so amply covered. He focuses on the years 1825 to 1860 and claims to be studying the "yeoman" ideal of success in presumed contrast to what others have written about. The contrast, unfortunately, is more rhetorical than real. The ideal of success that emphasized a "competence, independence, and morality" as opposed to strictly material riches is, by now, well known. The same can be said for related ideas that clustered around this theme. This book, then, offers us little new in the way of interpretation. It does, however, use certain new materials.

For example, Burns' use of mechanics' magazines in the antebellum period brings into focus a source less well mined by historians than children's literature has been. The attitudes toward success do not differ markedly from what we find in other places. There are, however, occasional glimmers of other attitudes that enlarge our view, such as the fascination with technology that characterizes these journals.

Burns' examination of Nathaniel Hawthorne's fiction from the perspective of the success ethic is also interesting. It illuminates Hawthorne's work in new ways and is a good example of the convergence of high culture and popular culture around this theme.

The book's last chapter is the least satisfying. Burns' attempt to carry his study into the late nineteenth century results in a handful of pages tacked onto the main body of the book as an afterthought. The author assumes that traditional success literature declined in the industrial era. Nothing could be further from the truth. The genre reached its peak in the years between 1870 and the First World War. Burns' failure to note this relates to his notion that success literature moved with the times. What characterizes the genre, in fact, is its stubborn resistance to change

and the ironic continuity of traditional themes during the heyday of industrialization. Ideas of success bear a more complex relation to industrial America than Burns' rather simple picture presents to us.

RICHARD WEISS  
University of California,  
Los Angeles

LAWRENCE GROSSMAN. *The Democratic Party and the Negro: Northern and National Politics, 1868-92*. (Blacks in the New World.) Urbana: University of Illinois Press. 1976. Pp. xi, 212. \$9.95.

The pre-Civil War Democratic party has been the subject of close study by some of the most eminent of American historians, but the postwar Democrats remain a mystery—faceless, racist foils to the much-studied Republicans. Lawrence Grossman looks at a central aspect of that neglected history—the relationship between northern and national Democrats and blacks from 1868 to 1892. Beginning with a too brief and somewhat superficial assessment of Democratic racial attitudes in 1868 (and a three paragraph summary of Democrats' pre-1868 racism—far too little to set the scene properly), Grossman continues with an able description of their painful adjustment to the new multiracial political environment in which they sought votes. Overcoming internal resistance, northern and national Democrats abandoned their unyielding hostility to racial, political, and legal equality, grudgingly acquiescing in the results of the war by 1872 and actively wooing blacks in much the same way they did other ethnic groups by the 1880s. Grossman argues, however, that this appeal to blacks was designed as much to prove the sincerity of the Democrats' new racial attitudes to white northern voters as it was to garner actual black support. In this, Democrats were successful, leading to what Grossman calls in his final chapter "The Paradox of Democratic Racial Policy." For since national and northern Democrats could at no time afford to break with their southern allies, the Democratic policy ultimately was one of accepting the rights guaranteed to blacks in theory and seeking black votes in the North on that basis, while resisting as unconstitutional all efforts to empower the national government to enforce those rights in the South. By reassuring northern whites of their commitment to fundamental racial justice and by demonstrating some black support, national and northern Democrats were able to take the race issue out of politics by 1892, despite continued racial oppression in the South.

Unfortunately, it was beyond the scope of the task Grossman set for himself to study a most important implication of all this—the implication that for one reason or another so many northern

voters were committed to fundamental racial justice in late nineteenth-century America that the Democrats had to assure them that they shared that commitment. That runs directly contrary to the commonly accepted view that northern voters had abandoned any lingering Civil War idealism by this time. Grossman's conclusions suggest that further study of politics and race in the 1870s and 1880s is warranted.

MICHAEL LES BENEDICT  
Ohio State University

HERBERT APTHEKER, editor. *The Correspondence of W. E. B. DuBois*. Volume 1, *Selections, 1877-1934*; volume 2, *Selections, 1934-1944*. Amherst: University of Massachusetts Press. 1973; 1976. Pp. xxv, 507. \$22.50 each.

To understand the century of the life of William Edward Burghardt DuBois (1868-1963) is to approach the very core of the despair and dignity, conciliation and confrontation, segregation and assimilation of the Afro-American experience. Such understanding is not yet possible because the major portion of DuBois' papers has been restricted since 1951. Francis Broderick (*W. E. B. DuBois, Negro Leader in Time of Crisis*, [1959]) was allowed access, but had only reached 1910 when the papers were closed. Elliott Rudwick (*W. E. B. DuBois: A Study in Minority Group Leadership*, [1960, rev. ed. 1968]) did not get "very much help" from his subject, although Broderick provided transcriptions of his DuBois letters. It is probably for this reason that these two painstaking biographies—the best available—have a certain "outside" quality, an almost legalistic reading of DuBois' published record for errors of fact and intent, and, occasionally, a marring iconoclasm (Broderick, pp. 16, 17, 175, and 182-83) and somewhat too contemporaneous imputation of racism to DuBois (Rudwick, 1968 ed., pp. 194-95). Neither offers more than a cursory evaluation of the post-NAACP career, the years after 1934.

Fortunately, this documentary *cul-de-sac* should soon be remedied. In 1973 the University of Massachusetts acquired the DuBois correspondence and papers (which Herbert Aptheker had been vouchsafed the task of editing in 1946), and in 1974 it received a grant from the National Endowment for the Humanities for archival support. Until this collection is ready, it is certainly useful to have Aptheker's two volumes (a third and final volume is projected) of selected DuBois correspondence, although the knowledge that DuBois may have written more than 100,000 letters (of which 1,135 are presented here) inevitably exposes the editor's achievement to controversy. DuBois "saved literally everything," Aptheker states. But these selections, drawn mainly from the University of Mas-

sachusetts collection, were determined by criteria excluding "practically all personal correspondence" and including "correspondence having significant historical and public quality."

Given the sheer mass of these papers, the dispersion of other letters among a multitude of repositories, and the special problems of funding an exhaustive DuBois edition, Aptheker may well be justified in offering a limited publication rather than a complete epistolary record in the manner of Louis Harlan's Booker T. Washington series or John Blassingame's Frederick Douglass series. It is, nevertheless, a puzzling offering because few scholars will choose to rely upon Aptheker's editorial judgment. (Presumably, access to this collection is imminent.) Indeed, the charge in Rayford Logan's introduction to his *W. E. B. DuBois, A Profile* (1971) that "suicidal" was amended in DuBois' posthumous autobiography to read "inadequate for our plight" in a key reference to Afro-American communism raises serious questions about Aptheker's ideological objectivity. Nor will his serious readers be content to know nothing of the personal thoughts of the editor's "significant historical and public" DuBois.

In our ignorance of what has been excluded, what is to be said of the correspondence presented by Aptheker? There are venial slips, such as the odd failure to identify the wife of Atlanta University President Rufus Clement (group photo, vol. 1:217); an erroneous speculation as to the identity of "Elizabeth" (vol. 2:181); and the unfortunate statement (vol. 2:219) that, after 1933, the first Mrs. DuBois "decided to live either in New York or Baltimore," when, in fact, she resided for two years on the Atlanta University campus. Of more consequence is that too many letters are longer than their relative importance appears to warrant (e.g., vol. 2: 8-10, 45-7, 148-50). The comparatively small selection of Pan-African letters (vol. 1) is surprising. Also surprising, despite the inclusion of several Charles Chesnutt letters and a characteristically umbrageous one from Claude McKay, is the dearth of correspondence from the Harlem Renaissance period.

Of enormous value are DuBois' letters and position papers on his concept of benign racial segregation (vol. 1), especially illuminating because of his early estrangement from Booker T. Washington and his 1934 rupture with the NAACP. Equally significant is the lengthy exchange (vol. 2) with his brilliant, radical disciple, George Streater, pertaining to racial segregation and Marxism. DuBois may not have been influenced by the writings of the Belgian socialist, Henri De Man, but, undoubtedly, he knew of them, and his hard-pressed explanations to the disillusioned Streater are redolent of De Man. The correspondence with Atlanta University President John Hope has a lu-



minous quality of intellectual and personal propinquity. And that with Hope's successor, the embattled Rufus Clement, who summarily retired DuBois at the very moment when the monumental *Encyclopedia of the Negro* project seemed possible, poignantly, communicates the plight of the visionary septuagenarian born before and, perhaps, living beyond his time. In light of DuBois's splenetic hatred of Walter White (vol. 2 contains two unusually bitter assessments of the NAACP secretary), it is to be hoped that the final volume will expose the underside of the abortive return to the NAACP (1944-48).

The arrogant/shy mulatto behind the larger-than-life Leader/Thinker, the DuBois whose extraordinary discipline and almost messianic vision struggled to make his life the vindication of an organizing principle for a debilitated, powerless race, necessarily eludes the student confined to published sources or even to the superb manuscript collections at Fisk and Howard universities, the New York Public, Library of Congress, and the smaller collections at Atlanta, Dillard, and Yale universities, and the Schomburg. The perceptive judgment of Charles H. Wesley that "DuBois was more than a historian; he was an advocate, a crusader, a critic, a teacher, a booster of morale" (J. H. Clarke, *Black Titan*, [1970], p. 94), as well as the three seminal DuBois chapters (9, 10, 11) in August Meier's touchstone *Negro Thought in America* (1963), wherein DuBois is described as "the epitome of the paradoxes in American Negro Thought" (p. 206)—scholar and mystic, racial solidarist and thoroughgoing integrationist, egalitarian and elitist—underscore why one of the most disabling deficiencies in Afro-American studies is the absence of a complete biography.

In addition to the works cited, there is much that is useful. Charles F. Kellogg's workmanlike NAACP history (1967) contains illuminating speculations about the DuBois-Villard feud. And again, Logan's trenchant introduction to his *W. E. B. DuBois* with its essays or excerpts reflecting a broad range of historians' assessments (especially Vincent Harding's black revisionist appreciation and Staughton Lynd's overdue, if overstated, defense of the magisterial *Black Reconstruction*) is indispensable. There is the two-volume compilation with distinguished commentary by Julius Lester, *The Seventh Son, The Thought and Writings of W. E. B. DuBois* (1971); Henry Lee Moon's informative introduction to his *Emerging Thought of W. E. B. DuBois* (1972); Daniel Walden's *W. E. B. DuBois: The Crisis Writings* (1972); Aptheker's *Annotated Bibliography of the Published Writings of W. E. B. DuBois* (1973), in which he was controversially assisted by Paul Partington; and, finally, the forthcoming *chef-d'oeuvre* by August Meier and Elliott Rudwick on the NAACP. But however meticulous

and instructive, none of these authors has benefited from unrestricted access to the papers from which Herbert Aptheker's two volumes are drawn. What we know of the "public" DuBois now makes us anxious to know a great deal more.

DAVID L. LEWIS

*University of the District of Columbia*

CARL R. OSTHAUS. *Freedmen, Philanthropy, and Fraud: A History of the Freedman's Savings Bank*. (Blacks in the New World.) Urbana: University of Illinois Press. 1976. Pp. 257. \$10.95.

This fine book throws clear light on a major promise to the freedmen made and withdrawn during the first decade following emancipation. Though richer in detail, the study does not greatly revise the interpretation of Walter L. Fleming in 1927. Fleming saw the black depositors as vulnerable because of their innate inferiority. Carl R. Osthaus ascribes the vulnerability to the lack of power of a newly-freed people. Both historians know the freedmen were exploited.

They were not, however, totally powerless. Indeed it took a lot of work by the white men who controlled the bank's investment portfolio to make the bank fail. "By 1870 the concept of a bank for ex-slaves had passed the experimental stage," writes Osthaus who finds that "credit for the Bank's success belongs to the freedmen." They put the money in the bank, from which more money could have been made. "Black cashiers, assistant cashiers, advisory boards, and churchmen, as well as white cashiers, assumed the leadership of the Bank in the Negro community, and black people responded." The black bourgeoisie displayed itself as estimable in the eyes of the white community, but it did not control its own bank.

The Freedman's Savings Bank was a fascinating institution. It was the only federally chartered interstate branch banking system of its kind in the nation's history. As a savings institution that could have created money for home, small business, and farm loans for a group with particular need for credit, it anticipated New Deal experiments seventy years later. The first problem of the bank, however, was that despite its Congressional charter and its advertising, it was not a public institution but a private one. And, in the Age of Capital, it was, finally, not a serious institution. Its object was philanthropy not profit, and when the philanthropists got in trouble in 1873, they unscrupulously moved bad loans from the First National Bank of Washington to the Freedman's Bank. It was this paper—notes of corporations in which the white trustees had invested—and not mortgages on freedmen farms or businesses that pulled the bank under.

When the ship was sinking, the trustees put a



black man on the bridge. Osthaus is more charitable than need be here. The new president, Frederick Douglass, was chosen not for his banking acumen but because he would restore confidence in the bank and keep the depositors from the sensible course of taking their money out. That money had been built up by the freedmen, making five and ten cent deposits, out of low cash returns on farm crops and small wages, so that they could enter the American middle-class world of ownership. When the bank failed they lost this chance.

The federal government did not make good the depositors' balances though it would have taken only a modest appropriation to do so. The author of this sensitive study speaks of the disillusionment of black people with banking institutions as a result. It would seem, however, that the most impressive lesson of the book is the opposite point, the immediate embrace of this most middle-class of institutions by the ex-slaves. They wanted to build an economic base above tenancy for themselves and rationally reached for a way to do so. It took two of the most pious crooks in American history, Jay Cooke and his brother Henry, to frustrate them.

There were models for successful savings banks. Banks like the Bowery in New York created capital from the deposits of immigrants and still remained cautious when confronting banking crises, like that of 1873 which hit the Cookes and the Freedman's Savings Bank. What could have been, under a trustworthy governmental guarantee, a splendid institution to insure an economic base for a good number of freedmen, failed. In the private realm, gentlemen bankers often rescue other gentlemen's banks, but no one saved the black people's bank of 1873.

WILLIAM S. MCFEELY  
Mount Holyoke College

GEORGE H. THOMPSON. *Arkansas and Reconstruction: The Influence of Geography, Economics, and Personality.* (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1976. Pp. xii, 296. \$15.00.

This is a strange book. The title would lead one to believe that it is a history of reconstruction in Arkansas, but it is not. The first three chapters, approximately one-fourth of the book, deal with the political geography of nineteenth-century Arkansas, secession, and the pardoning of political leaders at the end of the Civil War. Hardly a sentence is devoted to presidential Reconstruction.

Part two, almost half the book, is entitled "De Jure v. de Facto Government" and devotes a great deal of space to an arid and rather useless discussion of whether Arkansas conservatives regarded the Radical Republican governments of their state as legitimate. The Brooks-Baxter con-

troversy culminating in the so-called "war" is discussed at length, mainly from the point of view of two conservative state leaders, David Walker and Augustus H. Garland. Here the author has made a real contribution, although the concentration on personalities detracts at least as much as it adds.

A third section is entitled "Credit and the Development of Resources," and the first chapter goes back to the state-sponsored banks of the 1830s and describes their harmful effects upon the state's credit. Incidentally, the Panic of 1837 is nowhere mentioned in this discussion. Three chapters are then devoted to the construction and financing of the Little Rock and Fort Smith Railroad, and a short chapter deals with the state's repudiation of its prewar and Reconstruction debt. The author's conclusion, though very tentatively expressed, seems to be that Arkansas' poverty and generally poor financial condition since Reconstruction have been the results of the destruction of the state's credit.

The author states in his introduction that this study was originally an attempt to provide biographical profiles of prominent leaders during Reconstruction, which no doubt explains part of the book's lack of unity. Even in the biographical approach, however, there are strange omissions; Albert Pike's activity in the Masonic Order is emphasized, but his presumed prominence in the Ku Klux Klan is not mentioned, either for confirmation or denial. Isaac Murphy is worth only a few sentences, and Powell Clayton gets little more attention.

This book may prove instructive to readers interested in Arkansas history in general, but it most definitely is not a history of Reconstruction in Arkansas. In reality, it is three separate and almost unrelated essays. A history of Reconstruction in Arkansas free of the prejudices and biases of the 1920s is badly needed, but this is not it.

JOE GRAY TAYLOR  
McNeese State University

PETER D. KLINGMAN. *Josiah Walls: Florida's Black Congressman of Reconstruction.* Gainesville: University Presses of Florida. 1976. Pp. xi, 157. \$7.50.

Peter Klingman's *Josiah Walls* is a concise account of the turbulent life of a black man who rose to prominence in Florida politics during Reconstruction. Walls was born in Virginia in 1842, but it is unknown whether as a slave or a free person. Between 1868 and 1884, he experienced a spectacular rise, serving as a constitutional convention delegate, and for one term in the Florida house and two terms in the Florida Senate. He was brigadier general of the Florida Militia, mayor, county commissioner, newspaper publisher, editor, attorney,

and successful planter and landowner until the last twenty years of his life.

Although the author admits that this "is not a complete biography," he does an outstanding job of reconstructing Walls' life from scant newspaper accounts, congressional hearing, pension files, personal interviews, and a review of the National Negro Convention movement. Florida did not produce an outstanding black leader during Reconstruction, comparable to P. B. S. Pinchback in Louisiana, and one may say of the complex political arena of Florida that "no one black or white, had the key to its total control." Nevertheless, Walls did not passively accept Republican domination. He challenged, criticized, and debated his party's policies and leadership.

Similar to politicians in Louisiana, Walls' role in the Civil War provided a stepping stone for his entry into politics. During his unimpressive legislative career he urged educational facilities for blacks and whites, civil rights, and internal improvements, but few of these were approved. He pushed for similar legislation as a congressman. Allegations of frauds and challenges from fellow Republicans, and Democrats, cut his congressional career short. Disputed elections characterized Reconstruction politics in Florida and Walls lost both contested elections in which he was involved. Shortly after Reconstruction, his unsuccessful challenge to the Republican incumbent not only opened the way for a Democratic victory but spelled his political death.

Walls' personal life is examined by Klingman. He was married to his first wife for nineteen years, but her death, combined with the crop failure that resulted from the freeze of 1895, was the beginning of the end. His second marriage was cold and formal. Shortly afterward, he moved from Alachua County (Gainesville area) to Tallahassee where he was director of farms at Florida Normal School. Here he remained aloof, both politically and socially, until his death in 1905.

This is a well-written volume, reviewing the historiography of both Florida Reconstruction and black congressmen. Klingman's faults are few: the addition of maps of Florida, including the county and congressional areas from which Walls served would have added to the narrative. A more extensive description of other black politicians would also have placed Walls' career in better perspective and given us a chance to evaluate the unity among the few black politicians in Florida during Reconstruction. Taken as a whole, however, this is a splendid biography.

CHARLES VINCENT  
*Southern University*

E. MERTON COULTER. *George Walton Williams: The Life of a Southern Merchant and Banker, 1820-1903.*

Athens, Ga.: Hibriten Press. 1976. Pp. ix, 320. \$10.95.

For well over half a century E. Merton Coulter has been one of the most prolific of southern historians. While he has ranged widely over southern themes, he has a particular gift for rescuing from near oblivion southerners who never attained national prominence but who made significant contributions to their region. George Walton Williams was such a person.

Williams was born in North Carolina in 1820, but his family soon moved to the Nacoochee Valley in northeastern Georgia. After leaving there in 1838 with \$10.00, he entered into a successful mercantile partnership in Augusta with Daniel Hand. In 1852 the expanding business took Williams to Charleston, his home for the remainder of his life. Possessed of an "uncanny faculty for sensing financial and economic trends and thereby taking advantage of situations before others acted," he became one of the South's most successful entrepreneurs. His main interests were in wholesale grocery merchandising and banking, but he ventured into such other areas as real estate and railroads.

Compelled by a strong sense of civic responsibility, Williams chaired or served on scores of committees, and his substantial pledges touched off innumerable fundraising enterprises. One of his most successful efforts was providing relief during the Civil War for soldiers' families. Williams secured a pardon as soon as possible in 1865 and worked diligently to restore harmonious relations between the sections. He shunned public office except for several terms on the city commission, and he recognized that he was more skilled in organizing and directing groups than in public speaking. Williams' abilities, integrity, and willingness to work in the public interest earned respect and admiration far beyond the limits of Charleston. When he died in 1903 the South lost one of its most successful business and civic leaders.

LOWELL H. HARRISON  
*Western Kentucky University*

HUGH B. HAMMETT. *Hilary Abner Herbert: A Southerner Returns to the Union.* (Memoirs of the American Philosophical Society, number 110). Philadelphia: American Philosophical Society. 1976. Pp. xvi, 264. \$5.00.

Tackling the difficult task of rescuing an obscure character from historical oblivion, Hugh B. Hammett has exerted an heroic effort. Hilary A. Herbert, a Confederate colonel, Alabama congressman from 1877 to 1893, naval patron and secretary of the navy in the second Cleveland administration, historian and Bourbon spokesman, played a

series of supporting roles. Hammett properly relates Herbert's life and activities to larger themes of Reconstruction, sectional reunification, the New South, and the New American Navy.

Always conservative, always recalling "the good old antebellum days"—and for him they had been good—Herbert became an articulate romantic, a paternalist, a publicly active and typical representative of his class, and an important leader in the United Confederate Veterans. Interestingly, his writings reveal some of the same insights and assessments later to appear in the works of historians such as Jesse T. Carpenter, Allan Nevins, and Kenneth M. Stampp, but Herbert had more direct impact upon the efforts of James Ford Rhodes, William Archibald Dunning, and Walter Lynwood Fleming. Defensively interpreting and revering the Old South, Herbert also provided some leadership in a new era of national life. His most noticeable activity, by far, was in the area of naval affairs. He acquired an unsurpassed nautical knowledge in Congress, infused greater efficiency in naval organization and administration, and helped preside over the conversion to steam-powered vessels, the introduction of electric-powered gun turrets, and centralized design and building techniques.

Extensive research and commendably readable style make this book impressive and pleasant, but Herbert—his image still lacking warmth, color, and personality—remains a dim figure. He left prodigious papers, but there is too little trace of himself. Hammett concludes that Herbert had little significant impact upon the affairs of his day and that he understood those affairs little more than the average person of the nineteenth century. Yet he was there on the scene, through a long life of eighty-five years, exerting occasional influence, and no doubt swaying the passions of many people through his considerable speaking and writing ability.

HERMAN HATTAWAY  
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GERALD THOMPSON. *The Army and the Navajo: The Bosque Redondo Reservation Experiment, 1863-1868*. Tucson: University of Arizona Press. 1976. Pp. xi. 196. Cloth \$8.50, paper \$4.95.

This book is a well-documented administrative history of the controversial Bosque Redondo Reservation in New Mexico. Created for the Navajo Indians, the reserve was maintained by the United States Army for five hectic years before it was abandoned. According to Gerald Thompson, the reservation experiment was motivated by a humanitarian impulse to save the Navajo from exter-

mination and to "put them on the path to assimilation." The chief architect of the reservation, General James H. Carleton, is portrayed as a typical "mid-nineteenth century humanitarian" committed to re-creating the Indian in the image of God-fearing whites.

Despite the assertion of Carleton's humanitarianism, what emerges from Thompson's account is a revealing picture of a grandiose and mismanaged program of acculturation under duress. Thompson focuses on agricultural self-sufficiency and on the financial operations of the reserve. The agricultural program failed: never did the annual yields supply more than a small fraction of the food required. The total financial cost of maintaining the reservation approached \$10,000,000; poor business management, hoarding of foodstuffs by contractors, and outright graft contributed to the high cost. And despite the huge expenditures, the Indians suffered nearly constant privation. Significantly, it was the financial cost, rather than a recognition of the greater cost in human values, that led to the abandonment of the experiment.

Thompson calls the Bosque reservation experiment a "successful failure." He admits that it did not make the Navajos self-sufficient; nor did it end the depredations against them. He contends, however, that progress toward assimilation had been made, and this "success" convinced the government to proceed with the reservation policy. He also states that the experience "taught" officials "which pitfalls to avoid in starting and operating a large reservation." These claims appear debatable. Numerous civil and military officials, far from recommending the experiment, condemned it as a failure. And the subsequent ordeal of the Plains Indians on the barren reservations of Oklahoma and the Dakotas demonstrated an even more tragic failure than that suffered by the Navajo.

Such criticisms aside, this balanced study clarifies the nature of the confusion, corruption, and collapse of the regrettable venture at the Bosque. Unfortunately the copy sent to this reader is marred by the publisher's mistakes. These include errors in collating and missing pages of text.

MARVIN E. KROEKER  
East Central Oklahoma State University

RICHARD SALLET. *Russian-German Settlements in the United States*. Translated by LAVERN J. RIPPLEY and ARMAND BAUER. Fargo, N. D.: North Dakota Institute for Regional Studies. 1974. Pp. xii, 207. \$9.50.

Few people are aware of the presence of over 600,000 Russian-Germans in America. In the eighteenth century their forefathers migrated in several waves to Russia where their largest colony was in

the Volga region. For political and religious reasons they started their migration to the U.S. in the late 1800s. Here they formed some 1500 settlements, mainly in the prairie states, particularly the Dakotas. The author, Richard Sallet, a scholar and diplomat (who now lives in Spain), spent years among these people. He knows his subject intimately.

This book is an English translation of Sallet's original work published in German in 1931. The translators, Lavern J. Rippley (of St. Olaf College) and Armand Bauer have done an excellent job. Rippley also has written a very good introduction on the historical background of these immigrants while William C. Sherman, a sociologist, contributed a sketch about their rural prairie architecture. These additions combined with many interesting photographs, maps, statistics, and appendixes make this study absorbing, informative, and valuable to scholars. Was it really necessary, however, to include the long list of German settlements in Russia?

As farmers the Russian-Germans are extremely successful; their contribution to the agriculture of the prairie states is significant. For them America really proved to be a land of opportunity. The author describes this saga, dealing separately with Evangelical (comprising about two-thirds of the Russian-Germans) and Catholic groups from various parts of Russia. Was it necessary to divide them according to religion?

The book deals with the extremely important roles of the churches, press, clubs, and traditional customs. Highlighting the difficult World War I years, it stresses the inevitable problem of Americanization that faces every ethnic group. Even though a specialized study, it is a welcome addition in the recent upsurge of ethnic studies. With a good bibliography and extensive index, it sheds light on a relatively unknown subject in the ethnic history of America.

GEORGE J. PRPIC  
*John Carroll University,*  
*Cleveland*

HOWARD S. RUSSELL. *A Long, Deep Furrow: Three Centuries of Farming in New England*. Hanover, N.H.: University Press of New England. 1976. Pp. xvi, 672. \$18.50.

Howard S. Russell has been identified with agriculture for many years as an official of the Massachusetts Farm Bureau Federation, a lecturer on various topics, a historian, and a widely traveled observer.

The book's coverage of the three-and-a-half centuries of New England agriculture allots roughly forty percent to the Colonial period, forty percent to the period from the Revolution to the Civil War,

and twenty percent to the last century. There are fifty chapters more or less equal in length, which give the impression of having been designed as feature articles for farm journals or Sunday supplements or perhaps as occasional speeches. The approach is partly chronological and partly topical, so that there is a kind of spiral effect, with a certain aspect of farming or farm life being introduced and then treated on a recurring basis.

For the general reader, this history has many attractive features. It is clearly written, extremely fair in its evaluations, and generous in scope. The author is much less concerned with the farmer as a statistic than as a human being pitching hay, chopping wood, or driving cattle to market; he appreciates the role of the farm wife; and he knows the significance of schools, improved roads, and good local government. He is very successful in detailing the course of rural development over the centuries, not only in its triumphs and pleasures, but in its failures and discouragements.

The professional student of the evolution of American agriculture will notice that the author has used, in addition to the expected monographs and secondary works, a wide range of other materials, including an amazing number of local histories. At the same time, to judge from the references, he has placed little reliance on farm journals, and the bibliography reveals that only one of the agricultural reports of the Patent Office was consulted. While citations to authorities are somewhat random in pattern, and occasionally much too general to allow a check of sources, it is certain that the study is solid and dependable. The bibliography and the index are both excellent. There is a need for sound descriptive histories of the agriculture of the various American states or regions, and this is a satisfying example.

ROBERT LESLIE JONES  
*Marietta College*

ROBERT C. MCMATH, JR. *Populist Vanguard: A History of the Southern Farmers' Alliance*. Chapel Hill: University of North Carolina Press. 1976. Pp. xiii, 221. \$13.95.

*Populist Vanguard*, submitted originally as a doctoral dissertation and the first full-fledged study of the Farmers' Alliance in the South, is in many respects a solid piece of work that at times does more than justice to one of the most underrated farm organizations. The general picture is a familiar one, and the author unfolds it in greater detail at the local and state levels than at the national because of the nature of the organization. He reinforces the role the religious evangelicals played and brings what women there were in the movement to the fore. He also deserves high grades for extensive research; his writing is readable, and the



narrative moves along at a lively pace. Finally, his bibliography should prove useful to the advanced scholar as well as the general reader.

Although there is much solid material on the local and state levels, its abundance and the numerous names that emerge tend to lose the reader who is looking for that broader picture and meaning. At times one also has the feeling that the Farmers' Alliance functioned in a vacuum and often one begins to wonder just what has been said that has not been said before, even though it comes in greater detail. The noneconomic phases of the Alliance are given better treatment than the economic. Stating that "the southern Farmers' Alliance was until the rise of the American Farm Bureau Federation, the largest and most influential organization in the history of the United States" is saying too much. The author furnishes no evidence to support this. In 1977 the Grange will be celebrating its 110th anniversary and the Farmers' Union will celebrate its 75th. The Union once claimed a membership of 3,000,000, and its various business organizations through the years have done business in the billions of dollars; its persistence as a lobbying force in Washington has given it an influence far greater than its membership suggests. How many anniversaries did the Farmers' Alliance celebrate and how does its record of achievement and influence compare with that of the Farmers' Union and the Grange?

Robert C. McMath's study of the southern Farmers' Alliance, despite these limitations, is worthwhile reading. It is heartening indeed to see that in our urban-industrial age studies of our agricultural past are being pursued with vigor.

THEODORE SALOUTOS  
University of California,  
Los Angeles

EVERETT DICK. *Conquering the Great American Desert: Nebraska*. (Nebraska State Historical Society Publications, number 27.) Lincoln: Nebraska State Historical Society. 1975. Pp. xiii, 456. \$10.95.

Everett Dick has provided a volume designed for broader than local appeal. He cuts a wide swath both chronologically and geographically. Beginning with a survey of popular conceptions of the Plains as a "Great American Desert," he recounts the conquest over natural obstacles of scarce timber and water in provision for housing, fuel, fencing, wells, and agricultural production. To traditional accounts of this settlement effort he adds chapters on sorghum and sugar beet growing and shelter-belt development. Tales of the devastation of grasshopper invasions, the horrors of prairie fires, and the severity of prairie storms amplify the picture of regional hazards. Discussion of the com-

ing of the railroads, development of ranching, mechanization of agriculture, settlement boom of the eighties and its collapse in the nineties, and twentieth-century adjustments by dry farming and irrigation rounds out the chronological story. Sixteen chapters, supplemented with eighty-one pages of pictorial illustration, have presented the settlement phase of Nebraska history in grand proportions.

This is a volume to be read and enjoyed for its recounting of contemporary experience, rather than for analysis of historical development. As a reflection of the trials of life in the region the account is vivid. Dick's attention to technological detail and methodology is outstanding. He has utilized extensively the Burlington Papers at the Newberry Library, files of *Nebraska Farmer*, publications of the Nebraska Historical Society, local histories, and reminiscences, including a large number of personal interviews.

Conceptually, however, the volume depends primarily upon previous monographic publications of regional rather than local application. Apart from limited reference to the opening of the sandhills district, differentiation of settlement trends within the state is generally lacking. The bibliography does not include the study by Howard Ottoson and his colleagues on *Land and People in the Northern Plains Transition Area* (1966), which focuses on the peculiar situation of western Nebraska as a transition zone between prairies and plains. Numerous questions arise from the lack of analysis in specific geographical and chronological contexts. Dick rejects criticism of his earlier designation of the region as the "sod-house frontier" and emphasizes the widespread use of such construction, but he does not discuss the introduction of tar-paper shacks as a common alternative. Where, when, and why did this latter innovation occur? How commonly were wells "dug" to a depth of 280 feet (p. 103)? How general and at what levels is the water table which permits use of windmills for lifting water? Should sorghum production, which has long prevailed in remote rural areas of the southern Appalachians, be considered a specific adaptation to treelessness? Study of the areas characterized by dry farming and by irrigation is missing in this record of the "Conquering of the Great American Desert." The criteria which allot full-page consideration to methods of constructing an irrigation reservoir, while the agricultural adjustment program of the Soil Conservation Service is summarized in a prepositional phrase, based upon personal interview, are indicative of the strengths and weaknesses of this work.

MARY W. M. HARGREAVES  
University of Kentucky



HILDEGARD BINDER JOHNSON. *Order Upon the Land: The U.S. Rectangular Land Survey and the Upper Mississippi Country*. (Andrew H. Clark Series in the Historical Geography of North America.) New York: Oxford University Press. 1976. Pp. 268. Cloth \$9.00, paper \$5.00.

The locus of this study is a dissected, driftless, upland area comprising some thirty-eight counties along or near the Mississippi River from Galena and Dubuque to St. Paul. The author's geographical and historical interest in this hill country is the rectilinear line used there during the nineteenth century to divide the land and determine its appearance. She discusses state, county, and township lines, the lines of roads, fences, fields, and woodlots, the grid patterns within towns, and, influencing all the others, the lines laid down by the government's rectangular surveys. She fleshes out her statement of a geometrical phenomenon with such topics as land sales, grants to railroads, location of meridians and parallels, instructions to surveyors, descriptions of the land, and the opening, closing, and move westward of land districts and offices.

The dominant impression conveyed by this book is its solid content. Hildegard Johnson packs in the facts from surveyors' plats and notes, from state, county, and town histories and atlases, and from the professional literature of geographers and historians. But she overdoes this passion for learning when she recapitulates through many pages the familiar history of America's public land system. To broaden and enliven her story she cites Roman, Japanese, and Canadian experience in land surveying. And one of her chapters interprets nineteenth-century practice in the light of modern land management: strip farming occurs within the older rectangular surveys; the Whitewater valley in southeastern Minnesota becomes a game refuge. She cites modern artists who speculate whether mankind prefers to look at rectilinear or curvilinear lines. She is pleased to stress the flexibility of the rectangular system; a tract of 160 acres could take nineteen different shapes; rivers and ridges became county boundaries in the hill country; property lines adjusted to bluffs, terraces, valleys, and gorges. In her critical remarks on the land system she might have taken more seriously its failure to incorporate the best geodetic surveying of the day. How much disorder did this lack of science introduce into land transactions and legal affairs of the upper Mississippi region?

THOMAS G. MANNING  
Texas Tech University

NANCY ALEXANDER. *Father of Texas Geology: Robert T. Hill*. (Bicentennial Series in American Studies,

number 4.) Dallas: Southern Methodist University Press. 1976. Pp. xii, 317. \$12.50.

Since Robert Thomas Hill (1858-1941) has not been canonized in the *Dictionary of Scientific Biography*, a few words about him may help justify Nancy Alexander's biography. His immediate family scattered by the Civil War, Hill lived with relatives in Tennessee until at age sixteen he began work on a newspaper in Comanche, Texas. Intrigued by the Texas landscape, he studied geology at Cornell from 1882 to 1886. The United States Geological Survey then hired Hill and sent him back to Texas for fieldwork. Hill's years with the federal survey were his most productive—he created a stratigraphic system for the lower Cretaceous part of the geologic column in the Southwest, explored the Rio Grande, outlined the physiography of the Texas region, and described two major earth fractures in the state. His Texas studies enhanced the state's agricultural productivity, water supply, and petroleum industry, and his work on Texas rocks has been used by successive generations of interpreters. His importance could be highlighted only by a biographer knowledgeable about the geology and history of the state, in both of which Alexander is well qualified.

From 1894 through 1902, Hill studied West Indies geology, but in 1903 he resigned from the federal survey, and his scholarly output dropped as he sought monetary support and scientific facilities elsewhere. His business ventures were financial disasters, but government and private consultantships beginning in 1913 made his life less precarious. From 1931 until his death, he also wrote popular articles on geology and history for the Dallas *Morning News*. Alexander does not compare Hill's career to those of his contemporaries, such as Bailey Willis and Nelson H. Darton, leaving the reader to wonder at his uniqueness among scientists of that era.

With the publication of this book, the history of Texas geology has been examined biographically, as well as institutionally (Walter Keene Ferguson, *Geology and Politics in Frontier Texas, 1845-1909*, [1969]), and topically (Edgar Owen, *Trek of the Oil Finders: A History of Exploration for Petroleum*, [1975]). Alexander writes clearly and entertainingly for diverse readers interested in her topic—geologists, historians of science, and regional historians. Her work is based on Hill's papers and publications deposited at the Fondren Library of Southern Methodist University, supplemented with oral history interviews. Instead of composing a chronological narrative, Alexander devoted separate groups of chapters to three aspects of Hill's professional career, his study of Texas geology and geography, his work in the West Indies, and his business

enterprises. Alexander does not minimize Hill's combativeness and irascibility, even when those traits led him to personal vindictiveness toward colleagues. Without passing judgment, she also examines his attitudes toward various women in his life, toward minority peoples he encountered, and toward political controversies of his era. Having found only retrospective and fragmentary evidence on Hill's painful childhood years, Alexander avoids psychobiography, although she paints a vivid enough picture of his adult personality for those tempted to speculate.

MICHELE L. ALDRICH  
*Hatfield, Massachusetts*

GREGORY M. TOBIN. *The Making of a History: Walter Prescott Webb and The Great Plains*. Austin: University of Texas Press. 1976. Pp. xix, 184. \$10.95.

Almost from the moment of its publication in 1931, Walter Prescott Webb's *The Great Plains* was recognized as a "classic" interpretation of a prominent part of the American West. Not until now, however, forty-five years later, has a scholar turned to analyzing the provenance of that durable and provocative work. In this revision of his University of Texas dissertation, Gregory Tobin, an Australian who never knew Webb, gives every indication of understanding his subject thoroughly.

Tobin's purpose is to probe the relationship between Webb's "intellectual and social setting, the cultural matrix it supported, and a book that was at the same time both an attempt to define that setting and a product of it." This requires substantial biographical treatment and includes a fascinating chapter that reveals the intellectual debt Webb owed to Lindley Miller Keasbey, his teacher at Texas, who insisted on the need for a unifying theory in the writing of history and stressed heavily the impact of the environment upon human institutions. Later, when Keasbey came to believe that the environment was essentially passive, Webb chose to ignore his mentor's recantation because Webb's own experience with land and climate would not permit him to abandon his role as a staunch, virtually unreconstructed environmentalist.

Tobin makes very clear the quality in Webb that set him apart from and made him uneasy among academic historians: namely that *The Great Plains*, both in conception and method, grew out of a profound emotional reaction to environment and experience. That commitment forced a certain subordination of disciplined rationality and an indifference to, if not contempt for, exhaustive investigation. Thus Webb was vulnerable to attack from more conventional and professional historians such as Fred Shannon. All in all Tobin has written a judicious and perceptive assessment of

Webb and *The Great Plains*, his first and greatest achievement.

EDWIN R. BINGHAM  
*University of Oregon*

ROBERT C. TOLL. *Blacking Up: The Minstrel Show in Nineteenth-Century America*. New York: Oxford University Press. 1974. Pp. x, 310. \$10.95.

For over half a century blackface minstrelsy was the most popular entertainment form in the United States, playing a formative role both in the evolution of American show business and in embedding racial stereotypes into popular thought. The minstrel show emerged as common man's culture and in a nonthreatening way addressed itself to race at a time when white Americans were first forced to deal with the question of what position blacks would hold in a democratic society. Although minstrelsy viewed Negroes as inferior from the outset, most troupes expressed ambivalence about slavery before the mid-1850s. After that caricatures of happy slaves and discontented free Negroes dominated, while antislavery content virtually disappeared. Enjoying its greatest popularity in northeastern cities, the minstrel show also helped the new city-dwellers to cope with the bewildering uncertainties of urban life, establishing "a new sense of community and identity for them and their neighbors." Minstrels explained current events, new inventions, and scientific discoveries through the incompetence of their ethnic stereotypes, assuring white Americans that, no matter how inept they felt, Negroes were far worse off than they.

Robert Toll has written a well-balanced, analytical book stressing the social commentary behind blackface. He continues the story through the late nineteenth century, when white minstrels had shifted from portrayals of Negroes to lavish productions and a concern for the deterioration of moral values, the corruption of the young, and the disintegration of the family. By then black minstrels had entered the field, playing mainly to black audiences. Here Toll seems weakest, his analysis less sharp, even his narrative a bit mechanical. Yet on the whole *Blacking Up* is a praiseworthy study in popular culture, filling a void for an age that finds Carl Wittke's *Tambo and Bones* marred by objectionable racial attitudes.

RONALD L. DAVIS  
*Southern Methodist University*

WILLIAM O. REICHERT. *Partisans of Freedom: A Study in American Anarchism*. Bowling Green, Ohio: Bowling Green University Popular Press. 1976. Pp. x, 602. \$25.00.

*Partisans of Freedom* consists of a long adulatory survey of the writings of American anarchists. Its

tone may be discovered from its conclusion that the anarchists' "remarkable intellectual vitality . . . has found them riding the crest of every forward-looking social current of American life." Like many other books on anarchists and radicals, it discusses its subjects individually, first gleaning genealogical information from their memoirs, then pausing at length over ideas in their tracts and articles, and finally assessing their contributions to the anarchist tradition. Some, like Henry Clarke Wright, are nominated for "libertarianism's hall of fame"; others, like Benjamin R. Tucker, are chided for lapses from anarchist doctrines. One man's tolerant attitude toward Mormon polygamy is cited as an example of "the fact that the philosophy of anarchism equips its adherents to peer into shadowy depths which are often impenetrable to those with ordinary vision." Since the author borrows the anarchists' rhetoric instead of interpreting them historically, he directs much vehemence against cruel capitalists, incorrigible Marxists, and "that firebreathing monster, government." This approach makes even the discussions of Alexander Berkman and Emma Goldman seem tedious; it makes the anarchists as a group seem more ungenerous and dogmatic than they often were.

Reichert maintains that the anarchists have invariably held fast to a tradition of "free thought" fixed in the Enlightenment, and he lauds them for combating the lingering repressive power of Puritanism. He has difficulty with the evangelical beliefs of many nineteenth-century anarchists; even Paul Goodman, with whom he ends his study, appealed to the millenarian free spirit. But some anarchists were indeed so convinced that religious superstition lay at the root of injustice that they discounted the importance of economic forces—and even of the state. This glorification of "free thought" could result in curious versions of anarchism and, as Reichert shows, in diligent searches for a philosopher king in America.

It would be easy to compile from these pages a long list of errors and misconceptions. Even if the scholarship were more reliable, however, I doubt that some writers—Tom Paine, Randolph Bourne, A. J. Muste, and Dwight Macdonald—belong in the tradition Reichert defines. But *Partisans of Freedom* does provide sketches of many little-known anarchists, and by summarizing their writings Reichert has made available a compendium that other scholars may usefully consult.

LEWIS PERRY  
State University of New York,  
Buffalo

J. KIRKPATRICK FLACK. *Desideratum in Washington: The Intellectual Community in the Capital City,*

1870–1900. Cambridge, Mass.: Schenkman Publishing, 1975. Pp. x, 192. \$11.25.

In his "Essay on Sources," one of the more valuable parts of this study, Flack notes that the secondary literature on Washington scientific societies in the late nineteenth century is almost nonexistent. His book aims to remedy this. It is a flawed but informative account of associational activity among the city's professionals scattered along the fringe of political power. Cellular growth from early salons and clubs toward a federated structure of formal organizations, complete with bylaws, bulletins, directories and journals, is closely tracked. While an authentic literary community never developed, the high incidence of government scientists in the city's population underwrote strenuous efforts to marshall their influence as a professional, political, and social force. Joseph Henry's Philosophical Society of Washington and the Anthropological Society of Washington were the major new scientific organizations of the 1870s. Meanwhile, through the bold initiative of John Wesley Powell, the Cosmos Club emerged as a prestigious social rendezvous. Although the stubborn dream of a national university remained unrealized (for reasons not explored), a Washington Academy of Science was finally formed in 1898 to promote popular scientific awareness in the teeth of growing specialization among its several affiliates.

All this is described on the strength of enterprising manuscript research. Less successful is the attempt to establish a plausible context in which the historical meaning of one city's associational growth might be assessed. The expanding influence of Washington's elite across the Gilded Age is asserted but not demonstrated. Aside from a discussion of Powell's personal achievement in consolidating western survey projects under the U.S. Geological Survey, one learns little about the close interplay of science and public policy which presumably resulted from growing professional pressure on political decisions. Flack is sensitive to this issue, but does not systematically pursue it. And his efforts to sketch the social milieu of post-war Washington are vague, derivative, and sometimes incoherent. His pages flash with frequent insight into the problems of writing institutional history, but too often they read like a first draft. In bringing his work to print in its present form, he has been badly served by editor and publisher. Substance aside, his book is infested with errors of spelling, grammar, punctuation, usage, and typography. These make it the sloppiest scholarly publication the reviewer has ever read.

GEOFFREY BLODGETT  
Oberlin College

ROBERT SEAGER II and DORIS D. MAGUIRE, editors. *Letters and Papers of Alfred Thayer Mahan*. Three volumes. (Naval Letters Series.) Annapolis: Naval Institute Press. 1976. xvii, 718; xiii, 745; xiii, 873. \$95.00.

Editors Robert Seager II and Doris D. Maguire have searched for and discovered what must be nearly all of the extant product of the pen of this famed American navalist. In any event, one may *hope* that this is the last of it. Others may have produced drier prose, longer sentences, or more stilted English, but Mahan manages all three. We have "whereinsoever" and "discardment," an essay entitled "About what is the War," and one letter that begins: "In your editorial of Saturday, commenting on my first letter to you on the subject of the proposed immunity of commerce in war, you adduce the abandonment of the right to seize 'private' property on sea or land belonging to the one belligerent which at the outbreak of the war happens to be within the territory of the other, as indicating a tendency to the modification of the principle advocated by me, which principle, as you accurately enough state, 'is that injury to commerce is an expedient mode of waging war, and sound method of hindering war' " (II: 613). Mahan is generally quite clear, if read carefully and often, but he is rarely witty, graceful, or fun to read.

About a third of the materials published in these volumes appear to be the sort that were not easily accessible to Mahan scholars in the past. One new item, his 1868-69 diary, could have been written by a seventeenth-century Calvinist (see especially I:179). Some of the letters to his British friends are also revealing, as are the elaborate moral instructions he sent his daughters. And in this regard his daughters' recollections of Mahan are perhaps the most interesting pages in the three volumes (see especially III:723). Less significant, but sometimes rewarding, are his official correspondence and his letters to editors, publishers, and his attorney.

As is often the case, the portions of a famous person's correspondence that have been lost would appear to be at least as important as those that have been preserved. Consider the end of an otherwise uninteresting letter to a British friend in December, 1900: "I see we have difficulty at reaching the Chinese culprits [the Boxers] but I think they could be made to feel fear in the future . . . [remainder of letter lost]" (II:700; cf. I:355).

There are, of course, some noteworthy bits and pieces: Mahan "detest[ed]" the "purely dollars and cents view of national policy," but he was clearly aware that it "has its useful side" (II:452). A true conservative, he argued with the innovative navalist Washington Irving Chambers: "the trust in machinery has been pushed beyond reason. . . .

the living human factor is more and more relegated to a position hopelessly inferior[;] one must try, not to force the current back, but to deflect it somewhat" (II:76). He regarded Filipinos as "naturally treacherous" and spoke of "subject races" (II:693; III:6, 498, 596). He was obviously an irritable, snappy fellow (I:179; III:723). But we find in such fresh material little more than the confirmation of what we already knew, or at least suspected. There appear to be only a few minor editorial errors.

A reviewer ought not to scold those whom he reviews for failing to do something the reviewer wants done, and I do not fault the editors for their attention to Mahan. His importance may be greatly exaggerated, but he is probably interesting enough to have his correspondence thus assembled. Nonetheless, we have *all* said enough, for the time being, about naval commanders; we need to know more of those they commanded. Little is known of the nineteenth-century sailor, for most of us have ignored him, as Christopher McKee recently observed. In this regard, Mahan's brief, formal note to the Secretary of the Navy in August 1894, written while he was being ceremoniously honored by Britain's elite, becomes the most poignant missive in the set: "I have to report that on Saturday the eleventh instant while manning yards in honor of the Prince of Wales, who was visiting this ship, First Class Apprentice C. F. Hill in jumping from the rim of the fighting top of the foremast to the topmast rigging lost his hold and fell to the deck, receiving injuries from which he died eight hours later" (II:314). The U.S. Naval Institute, the publisher of these volumes, would do everyone a service were it to produce a collection of comparable quality on Mahan's men.

PETER KARSTEN  
*University of Pittsburgh*

H. G. RICKOVER. *How the Battleship Maine Was Destroyed*. Washington: Naval History Division, Department of the Navy. 1976. Pp. xv, 173. \$5.70.

Almost eighty years have elapsed since the USS *Maine* exploded and sank in Havana harbor with the loss of three-quarters of her complement. Since that time, historians have agreed that the disaster must be numbered among the immediate causes of the Spanish-American War, but responsibility for the explosion has been much more difficult to fix. Immediately, several naval officers and other qualified individuals expressed the opinion that it had been accidental, with some holding that the most likely cause was the spontaneous combustion of coal in a bunker adjoining a magazine. A Spanish investigation reached a similar conclusion, but an American court of inquiry in 1898 and a board of



investigation thirteen years later found "conclusive" evidence of an external cause. Their reports have tended to color American thinking on the matter since, although the absence of any information pertaining to the mining of Havana harbor has remained a troublesome factor.

Now, the indefatigable Admiral H. G. Rickover has turned his attention to the *Maine* disaster, writing an account of the international, political, and naval situations at the time and tracing the vessel's movements during the four months preceding her demise. After describing the personnel and proceedings of the United States Navy's official inquiries into the cause of the explosion, Rickover suggests reasons for their findings, which he asserts were erroneous. His conclusion is based on an appended analysis of the technical aspects of the *Maine's* destruction written by Ib. S. Hansen and Robert S. Price, specialists in ship structures and explosives, who studied the extant information, including the exhaustive textual and photographic records compiled by investigators when the *Maine's* hulk was raised to clear the harbor in 1911. The rather specialized content of this appendix is clarified by sketches illustrating the effects of the explosion. Other appendixes explore the international legal aspects of the warship's loss in a foreign harbor and include a report on the destruction of the French battleship *Îna* by internal explosions in 1907. Like Rickover's text, these are interesting and informative, but the Hansen-Price analysis must be considered the most important portion of the book. Its authors' conclusion "that an internal source was the cause of the explosion" with "heat from a fire in the coal bunker adjacent to the 6-inch reserve magazine" the most likely detonator is convincing. The explosion which destroyed the *Maine* is mysterious no longer.

ROBERT ERWIN JOHNSON  
*University of Alabama*

CHARLES S. CAMPBELL. *The Transformation of American Foreign Relations, 1865-1900*. (New American Nation Series.) New York: Harper and Row. 1976. Pp. xviii. 393. \$15.00.

Albert Bushnell Hart guided publication of the American Nation Series between 1904 and 1907. Taken together, its volumes in a sense defined the state of the historical art at that time. The same cannot be said of the still incomplete New American Nation Series. Appearing as they have over more than twenty years, the volumes in this series vary widely in character; they are built upon different bases and ask different questions. Thus Charles S. Campbell's *The Transformation of American Foreign Relations, 1865-1900*, makes its companion, Foster Rhea Dulles' *American's Rise to World*

*Power, 1898-1954* (1955), seem dated. Campbell's work rests largely, although by no means exclusively, upon a broad and extremely judicious reading of other historians, many of whom speak to different concerns from those upon whom Dulles perforce relied. Campbell's bibliographical essay, a model of its kind, is twice as long as Dulles', a rough indication of the proliferation of scholarship since 1955.

His two major themes, the author states at the outset, are the steady improvement of Anglo-American relations and the search for overseas territory. Campbell believes that proponents of territorial expansion, whether in the post-Civil War era or twenty years later, consistently sought the same objectives, "bases in the Caribbean and Hawaii, and a Central American canal" (p. xvii). The decision to take the Philippines, an anomaly but "a mistake hard to avoid" (p. 337), is viewed as essentially the almost accidental result of a purely military decision, the dispatch of Dewey to Manila Bay. However, if Anglo-American relations and expansion dominate the narrative they do not monopolize it; Campbell's coverage is comprehensive, extending to such things as political relations with Latin America, the causes of the Spanish-American war, and the growth of commerce, all treated at length, as well as others like the Maximilian affair, treated more cursorily.

If this volume has a failing, it is a failing of one of its virtues. Campbell is extremely judicious, raising and evaluating all sorts of interpretations. On occasion, however, as in assessing McKinley's role in 1898, the final verdict is so complex as to be blurred. On other occasions, Campbell tests various hypotheses essentially on their own terms without weighing one against others. Thus, for example, he assesses the various suggested contributions to the spirit of expansionism in the 1890s, commenting perceptively on each, but, as the editors of the series confess, he "does not seek to give precise weight to each of the components which underlay the new sense of urgency in the acquisition of overseas possessions" (p. xiv).

At other times, particularly in dealing with arguments usually identified with Walter LaFeber, Campbell is firm. He argues that "it is an oversimplification virtually to equate . . . two types of expansion" (p. 86), territorial and commercial, at least in part because their purposes may be so different. While he is well aware of the desire for new markets, particularly for manufactured goods, he shows how limited was government support and how mixed its success. He denies that a shift in business opinion played a major part in the coming of war with Spain. Finally, in a brief but forceful account of the Open Door notes of 1899, he argues that the administration put relatively little



stock in them and concludes, "the administration would have been astonished to know that its *modus vivendi* would some day be seen, by some writers, as partaking of the pure essence of imperialism" (p. 311). Such judgments make even more valuable a work whose comprehensive coverage in any event establishes it as the best survey of American diplomacy from the Civil War to 1900.

BRADFORD PERKINS  
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CALVIN DEARMOND DAVIS. *The United States and the Second Hague Peace Conference: American Diplomacy and International Organization, 1899-1914*. Durham, N.C.: Duke University Press. 1976. Pp. ix, 398. \$16.75.

In 1962 Calvin D. Davis won the Beveridge prize for *The United States and the First Hague Peace Conference*, a taut, fast-paced, and well-written discussion of early official American interest in permanent international organization. Now Davis has taken the story down to the First World War. Unlike his earlier book, *The United States and the Second Hague Peace Conference* surveys far more than American attitudes and behavior regarding events at The Hague. Davis' subject includes the Pan-American Conferences, the 1906 Geneva Conference on the Red Cross, the London Naval Conference of 1908, and the rules of warfare used in the First World War. In fact, Davis goes beyond the dates of his title and ends with a comprehensive account of what happened to the Hague idea at Versailles in 1919 and even includes a few cursory remarks about how the Senate finally approved the World Court proposal in June 1945.

Despite a comprehensive subject matter, *The United States and the Second Hague Peace Conference* is minutely detailed and documented with footnotes at the bottom of the page where they belong. Davis is at his best in unraveling the tedious discussions of men who had a passion for, of all things, international arbitration. He brings life to the conference of 239 delegates—from nearly all nominally independent states—which opened on a drizzly Dutch afternoon in June 1907. He knows how to break the monotony of legalistic discussions by quoting Mr. Dooley's lampoon of Andrew Carnegie, recalling the successes on the dance floor of a young secretary to the American delegation, or explaining how the Americans got a break on their hotel bill. Davis describes the embarrassment Secretary of State Elihu Root felt when he learned that one member of the hand-picked Cuban delegation had an anarchist past. The United States quickly forced this unlucky diplomat to recognize

that his health had suddenly given way, and he had to return home immediately to convalesce. That incident says as much about the conservatism of the advocates of international arbitration as it does about the presumed independence of some of the states represented at The Hague.

Early in his book Davis apologizes for having judged the first conference a failure in his 1962 volume. Now he is unwilling to apply higher standards to the men of The Hague than we demand of present-day diplomats. Davis refrains from dismissing as half-hearted or illusory the goals and actions of the international arbitrationists, even those that drew fire at the time. This reluctance to assess and criticize the legacy of the conferences at The Hague is the one major shortcoming of Davis' new volume. Davis does not group the advocates of the second Hague Conference according to their views on international organization, international relations, or American foreign policy. Accordingly, it is difficult for the reader to test the often-stated assertion that the movement for international arbitration and legal codification represented the most conservative wing of the internationalist movement. For answers to these questions readers must still refer to a book Davis acknowledges as a companion to his own, Warren Kuehl's *Seeking World Order: The United States and International Organization to 1920* (1969). Still, *The United States and the Second Hague Peace Conference* will stand for a long time as the definitive description of what happened in the Dutch capital in the summer and fall of 1907.

ROBERT D. SCHULZINGER  
University of Denver

HAROLD G. VATTER. *The Drive to Industrial Maturity: The U.S. Economy, 1860-1914*. (Contributions in Economics and Economic History, number 13.) Westport, Conn.: Greenwood Press. 1976. Pp. viii, 368. \$15.00.

This thoughtful volume is a praiseworthy effort to combine traditional economic history, with its narrative approach, and newer scholarship emphasizing quantitative evidence and mathematical models. Utilizing a wide variety of published materials, Harold G. Vatter attempts a synthesis of the crucial era between the point at which the United States committed itself decisively to a future of "industrialization, railroadization, and urbanization" and the emergence, at the time of World War I, of a "mixed capitalist economy" featuring significantly altered relationships between major interest groups.

A brief review can scan only a few of the many

issues Vatter covers. He generally supports the Cochran thesis that the Civil War temporarily "dampened the forces of long-run development," but highlights the importance of accompanying institutional and legislative shifts. In analyzing the postwar years he stresses the enduring significance of sectional differentiation and emphasizes the massive growth which occurred in the North Central states bordering the Great Lakes. While giving considerable attention to technological innovation and other changes affecting supply, he focuses particularly upon the forces augmenting demand and capital accumulation. He gives special consideration to the long period of declining prices from 1864 to 1896-97 and explains the failure of this trend to retard industrial development, though it did eventually modify the institutional setting by arousing political and social unrest. In a provocative chapter on economic relationships with foreign nations, he traces America's transition from "immature debtor" to "mature debtor" to "immature creditor" and supports the arguments of historians who have recently re-emphasized the economic motives underlying imperialism at the turn of the twentieth century after the closing of the frontier.

Vatter is to be commended for an impressive command of data, many acute judgments based upon a secure grounding in classical and modern economic theory, and skill in relating economic trends to social and political topics, including the rise of organized labor, the changing role of women, and persistent agrarian unrest. The bibliographical discussion at the end of the book is excellent. The volume, however, badly needs a summary chapter which might tie the many separate skeins of data it contains into something approaching an integrated thesis.

W. DAVID LEWIS  
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PHILIP S. BENJAMIN. *The Philadelphia Quakers in the Industrial Age, 1865-1920*. Philadelphia: Temple University Press. 1976. Pp. ix, 301. \$12.50.

Historians of the Society of Friends, focusing either on its dramatic early years or on its special witness to the war-torn twentieth century, commonly have neglected the Gilded Age. There is a profound gap between the ingrown quietist Quakerism of the antebellum years and the activist, cosmopolitan Quakerism expressed after 1917 in the American Friends Service Committee; the transition between the two was not an easy one for Friends to make. Their dilemma was acute: how to emerge from their separatist way of life enough to deal realistically with the problems of urban-industrial Gilded Age America, and not at the same

time lose all that had made their Quaker witness distinctive.

In dealing with the modern situation in which they found themselves, "conservative" and "liberal" Friends—the labels do not exactly fit—possessed complementary strengths and weaknesses. Orthodox Meetings, traditionally concerned for the American Indian, tended to neglect the immigrant at their Philadelphia doorstep in favor of the Indian in far-off Kansas—and, informed by that same tradition, were at the same time more knowingly critical of federal Indian policy than were Americans in general. Hicksite Meetings, on the other hand, quicker than the Orthodox to reach across sectarian lines and participate in the nascent Social Gospel movement in American Protestantism, were more ready than the Orthodox to compromise Quakerism's traditional peace witness during the First World War.

The author of this book was at home both in Quaker folkways and in the secular Philadelphia milieu. As well as the customary manuscript sources, he also drew upon the statistics of three central city Monthly Meetings. From those data it was possible to classify each of the members from two generations (those born in the 1840s and in the 1860s) as a Weighty, Practicing, or Nominal Friend. From that classification the author was able to draw some interesting inferences about the kinds of Friends that were active (or inactive) in various secular social concerns, and also about what was happening to Orthodox and Hicksite Friends generationally. This is a most useful kind of information to have, and it is unusual for any historian to combine this species of hard-headed empiricism with old-fashioned literary facility. *The Philadelphia Quakers in the Industrial Age* will surely stand as Philip Benjamin's monument.

PAULA A. CARTER  
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ROBERT F. DURDEN. *The Dukes of Durham, 1865-1929*. Durham: Duke University Press. 1975. Pp. xiv, 295. \$9.75.

The American Tobacco Company and Duke University are enduring monuments to the memory of the family of Washington Duke of Durham, North Carolina. Drawing from largely unused manuscript sources, Robert Durden analyzes the early growth of these two institutions amid the broader economic and philanthropic activities of the Duke family. In so doing, he provides a case study of the relationship between the rise of big business and the rise of "big philanthropy."

The Duke family is an excellent vehicle for such a study. Upon his return to North Carolina from the Civil War, Washington Duke proved to be an

able businessman, and his success in the tobacco industry established the foundation upon which his sons built an economic empire. In addition, the elder Duke was a staunch Methodist, and he began a tradition of philanthropy that his offspring also expanded. One son, James B. "Buck" Duke, became a central figure in the formation of the American Tobacco Company, one of the first of the giant trusts. Buck's older brother, Benjamin, played a somewhat lesser role in the management of the Dukes' businesses. Instead, until his death in 1929—the event that marks the end of this book—Benjamin guided the family's growing philanthropic activities. Emphasizing this division of labor between the sons, Durden's account is a history of a family unit, not a biography of the most famous member of the clan, Buck Duke.

This broad focus has its costs. Durden intersperses information on a variety of related, yet diverse, themes—internal family affairs; the expansion of the Dukes' tobacco, textile, and power interests; the increase in their charitable contributions; the influence of their activities on the economic development of a significant section of the New South. At times a confusion of purpose results. Those interested primarily in the history of business, education, or the New South will find in this narrative much useful information, but a sometimes frustrating lack of systematic analysis.

Those in each specialty should be interested in the account of the Dukes' charitable contributions. The family's altruism was initially the sum of each individual member's commitments to a variety of favored causes. As these gifts grew, however, so did the need for new institutional arrangements for their administration. To meet this need the brothers established the Duke Foundation in 1924. In tracing an important change in the affairs of a single family, Durden contributes to our general understanding of the process by which large-scale philanthropy became the function of institutions, not individuals.

The shift was of extreme importance to one of the Dukes' special clients, Trinity College. Beginning in the 1880s, the family took an ever-growing interest in the college. With the establishment of the Duke Endowment, this tie was formalized through a large endowment fund, and the changed relationship was embodied in the new name, Duke University. Its special tie to a dependable source of private funding initially allowed the university an independence of action and an ambition toward national excellence not afforded surrounding state-funded schools. Even in the 1920s, however, the limitations imposed by private financing undermined these ambitions.

In understanding the long-run implications of Duke's financial arrangements, the reader would

have benefited from comparisons to the numerous private universities similar to Duke that were established in roughly the same period. That this was not done reflects, in part, the general lack of useful comparative information available to Durden. Indeed, *The Dukes of Durham* provides one of the first efforts to see the benefactors of such private institutions whole, to connect their industrial activities to their philanthropic gifts. In a time when many private universities are facing the limits of their once envied endowments, Durden's study suggests the need for other similar works.

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ALFRED L. THIMM. *Business Ideologies in the Reform-Progressive Era, 1880–1914*. University: University of Alabama Press. 1976. Pp. viii, 264. \$9.75.

According to this book, the bedrock of business ideology in the nineteenth century was classical liberalism, particularly a belief in the continued viability of competition under the newly integrated corporate structures of the late nineteenth century. When this proved weak, it was strengthened by social Darwinism. The resulting amalgam, labeled "industrial capitalism," remained dominant until the last years of the century, when it was temporarily replaced by "finance capitalism," which emphasized the harmful effects of competition and the need for economic order. Unfortunately, capitalists failed to grasp the opportunities inherent in finance capitalism, and by 1920 the business community had returned to the "politics of irresponsibility."

Even if one grants the assumption implicit here, that economic organization is the only subject which merits extended treatment in a book-length overview of business ideology, the presentation of this thesis is deeply flawed. Although Alfred Thimm begins with a reasonable definition of ideology as the "rationalized class aspirations of business or business segments," the discussion of finance capitalism suffers from his own failure to take that definition seriously, to determine whether the anticompetitive core of finance capitalism was ideology, opinion, or an accurate description of reality. It is, moreover, misleading to call these anticompetitive ideas "finance capitalism," since one finds the genre everywhere in the 1890s and 1900s, in capitalists as classically individualistic and entrepreneurial as the printers of the United Typothetae.

The chapter on social Darwinism, and indeed the central place which Thimm assigns to social Darwinism in the history of business ideology, is

unfortunate on almost any terms. Thimm persistently indulges in the habit of reading survival-of-the-fittest views into every quotation on competition; repeats the error of attributing to Rockefeller, Sr. the famous American Beauty rose statement; fails to use Joseph Frazier Wall's recent study of Andrew Carnegie, which is excellent on this very question; and, as elsewhere in the book, insists on the representative quality of the statements of industrial giants (justified, the author would have us believe, because "the opinions of the local grocery store owner or small town banker are scarcely recorded").

This emphasis on social Darwinism is closely related to the author's virtually complete failure to understand or adequately probe the relationship between business and the state in the Progressive era. Believing, as he does, in the primacy of Spencer, Thimm explains the increasing participation of nineteenth-century businessmen in politics on the grounds that industrialists were utilizing "their newly found leisure," and Progressivism becomes (or should I say, remains) a movement to "protect the individual from the concentration of economic power." Ignoring the work of Gabriel Kolko and Robert Wiebe (among dozens of other historians in a variety of areas), Thimm can generate no plausible and consistent explanation for business interest and participation in economic regulation.

Beyond its conceptual weaknesses, this study is often dogmatic and inaccurate, as well as poorly organized and inadequately edited. There is room for a new study of business ideology in the Progressive era, but to be successful it will have to differentiate carefully between businessmen and exploit a wide variety of source materials of the very kind which Thimm asserts are unavailable.

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GERALD W. MCFARLAND. *Mugwumps, Morals and Politics, 1884-1920*. Amherst: University of Massachusetts Press. 1975. Pp. xi, 291. \$10.00.

Gerald W. McFarland begins by drawing a group portrait of the 1884 Mugwumps, describing their heredity, education, and occupations as well as their attitudes, ideals, and aspirations. He then follows these patrician reformers through the succeeding years in the Grover Cleveland wing of the Democratic party, in civic and social reform movements, and in the Progressive crusade. Central to Mugwumpery was a faith in professionalism that caused the Mugwumps to advocate civil service reform, made them organizers of numerous profes-

sional and reform societies, and led them to sympathize with the Progressive concept of the participation of experts at all levels of government. In describing these activities, McFarland provides readable and informative summaries of various reform activities, such as his good survey of urban reform in New York City in the 1890s.

Unfortunately, the inclusion of a number of very questionable quantitative features detracts from the qualitative merits of this book. Chapter two and appendix A compare various characteristics of 429 Mugwumps and 470 Republicans whose names appeared in New York City newspapers in 1884. Although designated as "samples," there is no evidence that these aggregations are representative of either the Mugwumps as a group, or the 90,000 Republican voters in New York City. Among other glaring examples of quantitative problems is a table (p. 75) labeled "Mugwump Party Preferences in National Elections, 1896-1916." The author again provides no information as to how these individuals were selected or whether they are representative of all Mugwumps. The 1916 "sample" consists of only 16 persons, even though appendix C provides a "masterlist" of 302 Mugwumps still living in 1910, and these 16 may or may not be among the 50 in the 1896 sample. In view of these problems, McFarland's contention that in 1916 Mugwump support for the Democrats dropped "below the 50 percent level again" remains unproven.

This book graphically illustrates the pitfalls confronting historians who attempt to apply quantitative methods to materials and inquiries better suited to a qualitative approach. It will be of interest primarily to those wishing to trace a particular individual's participation in various reform activities.

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FRED GREENBAUM. *Robert Marion La Follette*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1975. Pp. 275. \$8.95.

Now that the Robert M. La Follette papers in the Library of Congress are open for scholarly use, new studies of the great Wisconsin Progressive have begun to appear. The present account is part of the Twayne's World Leaders Series, under the editorship of Hans L. Trefousse. The volume, complete with notes, bibliography, and index, is well designed and printed.

Fred Greenbaum traces La Follette's life through his long and turbulent career. Born on a Wisconsin farm, he attended the University of Wisconsin, where he gained fame as an intercollegiate orator. He studied law, served as district

attorney, and was elected three times to Congress. After initial frustrations and a split with the state Republican leadership, La Follette won the governorship in 1900 and introduced such reforms as the direct primary, *ad valorem* taxation of railroads, a state civil service system, and a railroad commission. He also made use of university specialists to help solve some of the state's economic and social problems, a practice known as the "Wisconsin Idea." From here La Follette advanced to the United States Senate where he amassed an impressive legislative record in the public interest.

La Follette was a strong isolationist. He opposed U.S. entry into World War I, objected to the draft, and fought against the Versailles Treaty and the League of Nations. It was La Follette's inquiries which led to the investigation of the Teapot Dome scandal. Intensely ambitious, he repeatedly sought the presidency, but failed to win the Republican nomination in 1912, and was defeated in 1924 when he ran on a third-party ticket.

Greenbaum has written this short narrative biography principally for the general reader. Despite massive documentation he has provided little new information or critical analysis. The author views La Follette essentially as a failure (preface), an interpretation which both the subject and this reviewer would dispute. Many La Follette reforms, although not accepted when first presented, have since been adopted. Some scholars regard him as a precursor of the New Deal. Within its limitations, Greenbaum provides a well-paced account with a smooth style that should prove attractive to undergraduates and history buffs. However, a new critical full-length biography of Robert M. La Follette is still much needed. The present study is not that work.

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Nacogdoches, Texas

RICHARD M. LINKH. *American Catholicism and European Immigrants, 1900-1924*. New York: Center for Migration Studies. 1975. Pp. x, 200. \$7.95.

Ethnic diversity is a simple fact of American life, but how Americans have responded to this cultural condition is a very complex story. Richard Linkh's study which focuses on the response of the Catholic Church to the issue of ethnic diversity during the first quarter of the twentieth century illustrates the complexity of the issue.

Like many Americans, Catholic Church leaders were confused and divided on the issue of how they could best relate to the immigrant newcomers. Thus, from 1900 to 1917 they adopted a policy of benign neglect and let their immigrant co-religion-

ists shift for themselves in their adopted homeland. During this period the Church offered no intellectual leadership on the question of how the immigrants should adjust to their new cultural situation, and it provided little direct assistance to the newcomers. World War I, however, furnished the impetus for a dramatic change. Influenced by the rising tide of nationalism Church leaders adopted a vigorous assimilationist attitude. After 1917 the Church became "a major factor in immigrant welfare work and in 'Americanization' programs."

Linkh's argument is clear and convincing, but the manner in which it is developed is too drawn out. Too many chapters in the book are general surveys of a particular topic and offer little that is new. One solid article could have accomplished the task much better than a two-hundred-page monograph beefed up with a lot of stale information gleaned from well-used secondary sources. Too little rigorous analysis has also diminished the value of the study and the clarity of the argument. The absence of an index further compounds the problem of separating the wheat from the chaff. Linkh has chosen an important topic, but his study does not do it justice.

JAY P. DOLAN  
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WILLIAM IVY HAIR. *Carnival of Fury: Robert Charles and the New Orleans Race Riot of 1900*. Baton Rouge: Louisiana State University Press. 1976. Pp. xvi, 216. \$8.95.

In *Carnival of Fury*, William Ivy Hair has produced a well-written and dramatic account of an obscure Mississippi black man whose violent reaction against the caste system of the South in late July 1900 made him an instant hero among poor blacks of New Orleans and a notorious "black friend" among Southern whites. The son of a poor sharecropper in Copiah County, Mississippi, Robert Charles moved first to Vicksburg and later to New Orleans in a desperate search for a better and freer life. Unsuccessful in both places, he was attracted to the writings of Bishop Henry M. Turner and became an advocate of African emigration. Although not a violent man by nature, Charles was nonetheless a proud race-conscious individual who sought to maintain some substance of self-respect, and who refused to accept passively the indignities heaped upon him because of his race. As a result of these attitudes, he usually carried a pistol and was not averse to defending himself against insults.

On July 23, when three policemen questioned Charles and a companion about their presence late at night in a mixed neighborhood in New Orleans, Charles was not properly deferential. In an ex-



change of pistol shots, Charles was severely wounded in the leg but managed to escape in the darkness. After the police tracked him down at his room, he killed two of them before fleeing to a house on Saratoga Street where he hid from his pursuers for nearly four days before they learned of his whereabouts from a black informant. Before Charles was flushed out of his hideaway and completely silenced, he had killed five more whites and wounded twenty others. In the three day interval between shootings, race rioting, encouraged by sensational stories in the white newspapers, occurred on the streets of New Orleans in which several blacks were severely beaten and some were fatally shot.

Since Charles left no written records, Hair had to focus more upon the world in which he lived than upon the man himself. As a result, *Carnival of Fury* is not a traditional biography. Hair's descriptions of Copiah County and Vicksburg, Mississippi in the last half of the nineteenth century are excellent. In writing about life in New Orleans during the same period, Hair is at his best even though much of the material he includes is not germane to either Charles or the New Orleans race riot of 1900. For instance, Hair's excellent description of the "creoles of color" is extremely interesting but not especially relevant.

These minor criticisms aside, *Carnival of Fury* is an excellent book and may well become a model for other studies on little-known but significant black protest leaders.

ROBERT V. HAYNES  
*University of Houston*

ROBERT V. HAYNES. *A Night of Violence: The Houston Riot of 1917*. Baton Rouge: Louisiana State University Press. 1976. Pp. xii, 338. \$12.95.

In August, 1917, a mob of black soldiers from the Twenty-fourth Infantry entered the ghetto of Houston, Texas. In the ensuing riot four black soldiers and fifteen white civilians died. This is the point at which Robert V. Haynes begins his fascinating description of groups in conflict. The author examines the three groups whose actions led to the riot: white Houston (which was satisfied with the racial status quo), black Houston (which did not like the racial situation or the police but was powerless to change them), and the Twenty-fourth (which disliked Houston and the police, and could express their feelings). A police attack on Corporal Charles Baltimore let loose the soldiers' pent up frustrations. About one hundred well-armed blacks went in search of the police. After killing a few whites, the soldiers returned to camp and tried to hide their guilt in collective silence. The conspiracy did not work and the army

was able to gather enough evidence to court-martial 118 soldiers. Most were found guilty; many were given long prison terms; and nineteen were hanged.

This excellent book is more than just an account of a southern race riot. Using a variety of primary sources, Haynes explores the causes of the disturbance. He indicates why the characteristics of the Houston affair were more similar to the riots that would occur in 1919 than the riots of earlier years. The author's analysis highlights the contradictions between the concept of army service (citizenship) and the realities of army life for blacks (discrimination). This gap led to frustration and to violence. As Haynes indicates, black soldiers had responded in similar fashion to previous instances of discrimination. If there is any flaw in this book, it is in Haynes' examination of the influence of the Brownsville incident on the course of the Houston riot. The specter of the 1906 affray probably played a crucial role in the participants' decisions. For example, the men of the Twenty-fourth remembered that the silence of the Brownsville soldiers led to a light punishment; they had hoped history would repeat itself. A discussion of this process would have made the book even more illuminating.

Robert Haynes has written a fascinating account of how black racial pride, urban growth, and white discrimination led to a major race riot and the largest court-martial in American history. The book is both a gripping narrative and a sad commentary on discrimination in the United States.

MARVIN E. FLETCHER  
*Ohio University*

DAVID ALLAN LEVINE. *Internal Combustion: The Races in Detroit, 1915-1926*. (Contributions in Afro-American and African Studies, number 24.) Westport, Conn.: Greenwood Press. 1976. Pp. 221. \$13.50.

This slender volume is thoroughly researched and fairly well written. Its subtitle is misleading because it focuses mostly on only two aspects of race relations: housing discrimination and the inter-racial violence that accompanied it. After 1915 Detroit's black population expanded rapidly. Blacks were restricted mostly to unskilled industrial jobs, but the antagonism of many whites continued to grow. Attacks on Negroes who moved into all-white neighborhoods were frequent, and in 1924 the Klan's candidate for mayor came close to winning election.

In this atmosphere, black leaders practiced accommodation. David Allan Levine's data reinforces my belief that the Urban League was the most conservative of the black community organizations of that time and was often under the

thumb of white industrialists. The author goes too far, however, in stressing the "social control" aspect of the League to the exclusion of all other functions. Levine also discusses the Garvey movement. To my knowledge, he is the first to actually trace the occupations of local Garveyites, confirming the widely held theory that they were from the employed working class. The NAACP receives only passing mention in a brief "epilogue."

Unfortunately, this study has major flaws. For a book that covers only a ten year period, its focus is narrow. It does not, for example, deal at all with patterns of discrimination in schools or public accommodations. The book is disjointed and does not flow smoothly. Chapter two skips from a discussion of housing conflicts and violence after World War I to a retrospective glance at the early history of black Detroit (especially its elite) to a generalized inquiry into the causes of the Great Migration of 1916-19. Too much time is spent on diversionary material, such as the careers of James Couzens and Clarence Darrow.

The author's episodic style also impairs attempts at theory. He contends that the takeover of Detroit's government by reformers eliminated the nascent boss system and, as a result, "the overcrowded and the fearful" now had "no institutional outlet for their discontents." One consequence was contested neighborhoods, culminating in the violence of the 1925 Ossian Sweet incident. Yet Levine's own evidence does not seem to support this improbable theory. Violence against black homeowners apparently occurred mostly in those elite areas most prone to *favoring* reform. And if reform and racial violence are related, how then can one explain boss-ridden Chicago, which had even more racial incidents than Detroit in the 1920s?

While it contains useful information and interesting speculations, then, this volume falls considerably short of Elliott Rudwick's study of East St. Louis or William Tuttle's volume on the Chicago race riot in explicating the causes of racial conflict.

KENNETH L. KUSMER  
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JOSEPH ELLIS and ROBERT MOORE. *School for Soldiers: West Point and the Profession of Arms*. New York: Oxford University Press. 1974. Pp. ix, 291. \$9.95.

West Point, with its controversial "silent treatment," its recent admission of women, and its recurring cheating scandals, continues to make contemporary headlines. Joseph Ellis and Robert Moore are two former Army captains and instructors at West Point, with respective specialties in history and English. Their own education came not at the Academy, however, but in undergraduate and graduate institutions with strong lib-

eral arts traditions. Using their personal knowledge of both West Point and civilian universities, they analyze the methods and end results of the Academy's educational effort.

The authors' emphasis is on the Academy's mathematical and engineering approach to instruction, an approach that calls for a vigorous attack on the problem, its breakdown into manageable components, and its final definitive answer. This organizational approach develops methodical work habits, strict attention to detail, and respect for order and authority. Clearly, however, it is not the same as a liberal arts education, where intellectual curiosity, specialized knowledge, individual pursuit of learning, and consideration of unorthodox ideas are prized. West Point, in the authors' views, trains officers to think they know or can find all answers; liberal arts colleges teach students to consider questions to which there may be no answers.

Throughout their study of West Point, the authors make a strong effort at objectivity in their statements and judgments, and back up the text with exceptionally comprehensive notes at the end of the book. Their use of interviews is particularly effective in providing the "feel" of the institution. In summation, *School for Soldiers* is the fairest and most incisive contemporary consideration of the Academy this reviewer has seen.

Objectivity does not mean the authors have no opinion; despite the lack of polemics, their final view of West Point is negative. The purpose of West Point, with such extreme and stress-filled procedures for the new cadets as Beast Barracks, is professional socialization. Its purpose is training, not education; thus, the Academy is not the first-rate academic institution that the authors clearly wish it to be. Though one can sympathize with the obvious academic frustrations of Ellis and Moore and agree with their conclusions concerning the Academy's equally obvious educational shortcomings, I am still left with the unanswered question as to how a shift away from the structured socialization process—a move the authors undoubtedly desire—toward a more flexible liberal academic environment would better fulfill the mission of that institution on the Hudson: preparing professional officers for the Regular Army.

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Dallas

JEROLD S. AUERBACH. *Unequal Justice: Lawyers and Social Change in Modern America*. New York: Oxford University Press. 1976. Pp. xiii, 395. \$13.95.

*Unequal Justice* is a significant and scholarly flowering from the seedbed of the sixties. Concerned with

the problem of persistent social inequality, Jerold S. Auerbach examines the ways that the legal profession has failed to alleviate, and frequently worked to compound, oppressive social conditions. He focuses, too, on the deceptive nature of such legal ideals as professionalism, value neutrality, and technical craftsmanship. Identifying those ideals as "merely an expression of different, but equally partisan, values," the author proclaims his own commitment to "that equality of justice under law which those values persistently subvert."

A social history of the elite bar from the late nineteenth through the mid-twentieth century, the book charts the rise of the corporation lawyer and the national law school professor to positions of professional dominance, and examines how the two groups have responded to the challenges of social and institutional change. Auerbach's analysis is thoughtful and persuasive; his conclusions harsh but justified. The "professional" standards that elite lawyers upheld were largely self-serving; they functioned to ensure the continued dominance of elite groups and to protect the economic status quo. Insofar as those standards proclaimed the ideal of equal justice, the bar consistently failed to live up to them. The bar itself was kept stratified along class and ethnic lines, a stratification which tended to restrict severely the opportunities of "newer" ethnic groups, to bestow professional sanction on the power of private wealth, and to prevent the lower and middle classes from receiving satisfactory and effective legal representation. The author gives due attention to structural changes within the profession, emphasizing, for example, the ways in which the New Deal expanded and reoriented the legal elite. Still, however, the changes were mainly *plus ça changes*.

It is unfortunate, though unavoidable, that the book is unable to deal comprehensively with the wide range of problems it raises. This merely reflects the breadth and significance of the general—and too generally neglected—subject. Auerbach's major contribution may not rest on any specific findings, but rather on the fact that he has helped open up important areas of investigation and demonstrated that "outsiders" (non-lawyers) may be fully qualified to understand the social significance of the legal process. More significantly he has also shown that such "outsiders" are essential in order to move the analysis beyond the narrow and partisan level established by the rhetoric of the organized bar. We shall all be better off, in scholarly as well as political terms, if other historians follow the author's able lead and begin to explore critically the social and political function of the legal profession in the United States.

EDWARD A. PURCELL, JR.  
*University of Missouri,  
Columbia*

SAMUEL E. KARFF, editor. *Hebrew Union College-Jewish Institute of Religion At One Hundred Years*. New York: Hebrew Union College Press. 1976. Pp. xviii, 501. \$20.00.

Although this book is a "house" history and has many of the weaknesses of the genre, it is, overall, an interesting and workmanlike study of the famous Reform Jewish educational institution whose development has mirrored the many changes in American Jewish life during the last hundred years.

When the Hebrew Union College was opened in October, 1875, for example, its founders, along with most other American Jews, were opposed to political Zionism. By 1975, however, a branch of the combined Hebrew Union College-Jewish Institute of Religion had been established in Jerusalem and the college, as well as the majority of American Jews, firmly supported the state of Israel.

Similarly, at its inception and for many years thereafter, the German-Jewish businessmen who provided the backing and support for the college wanted to create a rabbinate who would continue the secularized Judaism which had originated in their homeland. As a result, the Hebrew Union College did not have scholars with a more traditional Eastern European background on its faculty until well into the twentieth century. This attitude reflected the deep antagonisms which divided the American Jewish community for many years.

Although somewhat limited by the need to provide practical training to future rabbis, the College-Institute has prided itself on the scholarly achievements of its faculty. For this reason, the second half of the book is devoted to the accomplishments of the teaching staff, both past and present. Because much of this material is included in the narrative portion as well, the "*Festschrift*" segment makes the book less readable. The copious notes, however, are of value and the seventeen pages of photographs add a valuable dimension to the text.

SELMA C. BERROL  
*Baruch College,  
City University of New York*

A. MCGEEHEE HARVEY. *Adventures in Medical Research: A Century of Discovery at Johns Hopkins*. Baltimore: Johns Hopkins University Press. 1976. Pp. xiii, 464. \$16.95.

Johns Hopkins stands alone among medical schools in its profound impact upon the development of American medical education. One of its basic contributions was the principle that laboratory and clinical research should constitute a major aspect of medical education; hence it is fitting

that one of its distinguished professors should write about the work of Hopkins' professors and students in medical science. One cannot help being impressed by the contributions to science of the great and near-great at Hopkins. Yet modern discoveries are largely the result of a great many scientists in diverse areas patiently accumulating bits of knowledge. Brilliant individuals occasionally anticipate the generalizations stemming from these accumulations, but science is essentially a collective effort. I mention this since any history of research which concentrates upon a particular institution necessarily gives an incomplete picture. Which particular discovery in virology or tissue culture is more significant than the host of others is largely a matter of judgment. I say this not to denigrate Hopkins, but merely to point out that other institutions, too, can lay claim to a multitude of important scientific "firsts."

If Hopkins stands alone in its impact on medical education, it may also stand alone in its ability to instill a sense of loyalty in its faculty and students. The school opened its doors in 1893, and it is already the subject of a three-volume history which carries the story to 1914 and another volume covering from 1914 to 1947. Major biographies have been written of several faculty members, and there is scarcely anyone connected with the school for whom some publication is not available. A. McGehee Harvey, in the course of discussing research, presents a thumbnail sketch of each clinician or laboratory worker, and through his intimate knowledge of the institution, he supplies a good many interesting anecdotes. Whatever else may be said of Hopkins, it has no unsung heroes—or non-heroes either.

JOHN DUFFY  
University of Maryland,  
College Park

RONALD L. NUMBERS. *Prophetess of Health: A Study of Ellen G. White*. New York: Harper and Row. 1976. Pp. xiv, 271. \$10.00.

Ellen G. White (1827–1915), prompted by dramatic daytime visions beginning in 1844 during the height of expectancy of Millerite millennialism, set forth on her own distinctive course that led to Seventh-Day Adventism. Although mainly concerned with the imminence of Christ's return, the prophetess placed great and continuing emphasis upon instructing her faithful how to pursue health during their premillennial days. Focusing on these health themes as seen within the broader concerns of Ellen White's life, Ronald L. Numbers has written an excellent case study in the affinity between unorthodox religion and heterodox medicine.

Brought up in the Adventist faith, Numbers presents an understanding but not uncritical study, based on family manuscripts and church records hitherto closed to researchers. While deliberately abstaining from "extended analysis of" Mrs. White's "mental health and psychic abilities" (p. xii), Numbers does not use "divine inspiration as an historical explanation" (p. xi). Indeed, although Ellen White insisted that her visions concerning health came only from God, Numbers employs parallel columns to reveal the striking verbal similarity between the transcribed visions and the published writings of popular health reformers.

Antiestablishment ideas of how to pursue health attracted Ellen White: Sylvester Graham on diet; James Caleb Jackson on hydropathy (the Whites twice took treatments at Jackson's Home on the Hillside, where they also encountered phrenology); Larkin B. Coles on the perils of self-abuse and excessive sex in marriage; Russell T. Trall's assorted unorthodoxies (Ellen's two sons and John Harvey Kellogg received diplomas after brief attendance at Trall's Hygeio-Therapeutic College).

Ellen White's doctrines centered upon what she deemed Nature's way: exercise; rest; fresh air; two meals a day; a vegetarian diet (after some wavering, a firm principle following 1894); an avoidance of alcohol, tobacco, tea, coffee, stimulating foods; a reliance on hydrotherapy instead of drugs; simple clothing, including a modified Bloomer costume for women; extreme sexual restraint. First at Battle Creek, and, before Ellen White's death, all over the world, health institutions embodying such doctrines served the faithful and sought to win converts.

If Numbers' fine book has any fault, it is in failing to convey adequately the charisma that Ellen White must have possessed to permit her, aided by her husband's talents at administration and publicity, to overcome considerable opposition to her health ideas and fasten them as articles of faith upon her expanding body of disciples.

JAMES HARVEY YOUNG  
Emory University

JOHN FINBAR JONES and JOHN MIDDLEMIST HERRICK. *Citizens in Service: Volunteers in Social Welfare during the Depression, 1929–1941*. East Lansing: Michigan State University Press. 1976. Pp. xvii, 137. \$8.75.

This study of voluntarism in the Depression seeks to understand the "meaning of citizen participation in the context of a planned society." Purporting to concentrate on the mass of volunteers who provided cars and drivers, clerical help, initial intake interviews—in short, the routine leg work on which social welfare agencies depended—the au-

thors promise a history of New Deal voluntarism from the inside out. *Citizens in Service* represents a missed opportunity, for such a tale could be told. John Finbar Jones and John Middlemist Herrick scoured archives far and wide, including the National Archives and the monumental University of Minnesota Social Welfare History Archives to present snippets of the story of a variety of private and public agencies like the Red Cross and the Federal Emergency Relief Administration. Organizations participating in welfare measures served some 25 percent of the population in the United States during this fateful decade.

The book ranges widely through the government alphabet agencies, the YMCA and YWCA, Big Brother and Sister organizations, the Child Welfare League of America, the Junior League, and Family Welfare Associations. But in this instance more is not better, for an interpretation of voluntarism from 1929 to 1941 might have been easier to formulate by concentrating selectively on fewer groups. The most interesting materials in the study emerge when the authors focus in detail on specific agencies like the Red Cross and the Baden Street Settlement in Rochester, or concentrate on individuals like Mabel Boardman, a stalwart of high society, National Director of Volunteer Services for the Red Cross, who fought against the professionalization of the agency. According to her, this trend would mean men taking over high principal offices and allocating work in the field to a group of community people. At the national level, the Red Cross was drawn reluctantly into the field of general relief. But at the local stage, the organization moved quickly and firmly from drought relief to "plain unemployment relief." Here was a case of local voluntarism and the crux of the missed story of voluntary activity. For voluntarism at the grass roots is not always apparent from official directives, on which the book largely depends.

The book concludes that 1932 marked the entry of the federal government into the sphere of social welfare; but 1912 is actually a far better date, for that year commemorates the founding of the Children's Bureau in the Department of Labor and Commerce. Facilitated by volunteers across the nation, the Bureau achieved such significant social welfare measures as uniform birth registration, the reduction of infant mortality, and the protection of the dependent child. The agency itself represents a fascinating case study in the function and use of voluntary methods in the heart of a government bureaucracy, antedating New Deal experiments in citizen participation by more than a generation and continuing these techniques through the Depression. The history of the Bureau in the late twenties and thirties relates to the authors' study

in another sense, for here is a chapter in the developing tensions between social welfare professionals and volunteers. It is unfortunate that the authors excluded the Bureau as an area of inquiry.

In order to understand the contours of voluntarism we need to know more about the connections between informal voluntary networks like the General Federation of Women's Clubs and PTAs as well as formal welfare organizations, and to ask the question slighted in this treatment—what voluntarism represented to the volunteers, the majority of whom were women.

The authors indicate their work contains gaps for others to fill. Social historians are thus alerted to the canyons in the social welfare landscape awaiting exploration.

NANCY P. WEISS  
University of California,  
Los Angeles

ARTHUR FRANK WERTHEIM. *The New York Little Renaissance: Iconoclasm, Modernism, and Nationalism in American Culture, 1908–1917*. New York: New York University Press. 1976. Pp. xiii, 276. \$16.00.

Arthur Frank Wertheim has produced a welcome survey of the so-called "Little Renaissance," that vital decade between about 1908 and 1917, during which a fresh new American culture faced up to the twentieth-century, tentatively perhaps, but with real courage. Why it should ever have been named the "Little Renaissance" is not entirely clear, except that it was short, but there is no denying the richness and vigorous creativity in all of the arts of the period. Though the germinal importance of that decade has long been recognized and the literature concerning it is quite extensive, this is the first attempt to describe its entire range of cultural and artistic activities, people, and their interconnections, which is both its virtue and fault.

Wertheim has made a prodigious effort to get all the facts recorded, related, and explained. It would be impossible in a short review to cover even in outline the personalities, groups, and movements, the magazines, theaters, and art exhibitions that are discussed. He has focused his attention on New York City as the cultural center which after the turn of the century attracted the major literary and artistic talents away from Chicago and Philadelphia. He has described the economic, political, and social background that produced the intellectual ferment of those years and pictured the variety and complexity of cultural life in the metropolis. Wertheim has not missed a detail, from the radical *Masses* to the apolitical *Smart Set*, from Mabel Dodge's salon (though he insists on



calling her Mrs. Luhan) to the Wobblies. There are careful summaries of the role of the *Seven Arts* and the *New Republic* in relation to American literature and of the small magazines—*Rogue*, *The Glebe*, and *Others*—in fostering the new poetry. The little theater movement is also described through the history of the New Theatre, the Neighborhood Playhouse, the Washington Square Players, and the Provincetown Players. Comparable events in the art world are documented from the exhibition of *The Eight* in 1908 (though he is less than clear in distinguishing the group from the Ashcan School) through the Armory Show of 1913 to the first Independent Show in 1917. There are detailed accounts of Alfred Stieglitz and 291, the Arensberg circle and New York Dada, as well as the emergence of modernism in American art. And in addition, there are conscientious treatments of a variety of social and cultural issues—socialism, anarchism, and bohemianism, experimental education, Freudianism, sex, and feminism. Finally, behind it all, is a serious effort to describe the historical reasons for all these developments.

The “Little Renaissance” is presented as a revolt against the genteel tradition of the late nineteenth century motivated by “the ideas and spirit of iconoclasm, modernism, and nationalism.” I am in almost total agreement with Wertheim’s viewpoint and analysis. My criticism is that the result is an excellent record, but not a very readable book. It may be that too much detail is crammed into too little space. In any case, the color, the creative vitality, the youthful excitement, even the retrospective nostalgia of the period are somehow lost. A marvelous moment seems to have been embalmed in a doctoral dissertation. And although the coverage is extensive, in many cases (especially the visual arts) it seems to me too dependent on secondary sources and opinions. However, even with its shortcomings, the book is a commendable effort and should be an invaluable guide to the entire range of cultural activity during the years of the “Little Renaissance.”

MILTON W. BROWN  
City University of New York

GARTH JOWETT. *Film: The Democratic Art*. Boston: Little, Brown and Company, for the American Film Institute. 1976. Pp. xx, 518. \$19.95.

Garth Jowett should be read in concert with Robert Sklar’s *Movie-Made America: A Cultural History of American Movies* (1975) for, as he admits, a complete social history of this broad field is impossible. Yet, despite his demurrer, Jowett has written a fine book.

He finds cinema, coming as it did on a wave of urbanization and industrialization, a destroyer of regional and local social constraints and a threat

to traditional agents of social control. Therefore, Jowett argues, urban progressive reformers, clergymen, and educators called for censorship—federal, local, or religious—as a means of restoring their primacy over the health, education, and morals of Americans. The half-century conflict between reformers and filmmakers resulted in a form of self-regulation that shaped American movies and audiences for three generations.

Like any good surveyor, Jowett is at his best when previous scholars have done sound work. He depends on Nicholas Vardac, Russell Merritt, Terry Ramsaye, and others who have studied the early complexities of class tastes in film, neighborhood patterns of theater-building, and the process by which film swallowed theater. His work also shines when he takes up cinema during the world wars, when public agencies produced data-packed reports and the issue of censorship generated a stream of informative polemics.

Nevertheless, we should want to know more about the personalities of individual studios; Hollywood as a “company town”; Yiddish films; “race movies” for black audiences; experimental film; differences in regional and class tastes; and the impact of external forces such as economic trends and corporate rivalries. And, in addition to Jowett’s rich bibliography, we could wish for a sifting of accessible corporate records, manuscripts of filmmakers, interviews, audiences’ responses recorded on preview cards, regional newspapers for their varied advertising, reviews, and readers’ letters, and, most of all, the correspondence of local bookers and exhibitors in trade papers. Yet the call for such labor must be heard as a plea for more monographs rather than a charge of want of diligence on the part of the author of this important and useful book.

THOMAS CRIPPS  
Morgan State University

JOHN MORTON BLUM. *V Was for Victory: Politics and American Culture during World War II*. New York: Harcourt Brace Jovanovich. 1976. Pp. xii, 372. \$12.95.

John Morton Blum pointedly explains that *V Was for Victory* is neither a history of the American people during World War II nor a chronicle of the homefront. Instead, Blum has chosen to focus on some particular aspects of American politics and culture during the conflict that interest him and to suggest some of the ways that politics and culture interacted. Such a format obviously runs the risk of being seriously disjointed and unbalanced, but Blum’s book is neither. He has produced a fascinating mosaic, engagingly written and characterized by penetrating observations.

Despite his eclectic approach (“War Bonds and

War Aims," "Jim Crow," "The War in Literature," "Pains for Small Business," "Jews in Great Britain," etc.), Blum does develop a basic theme. Republicans and their southern Democratic allies, he declares, "intended to wipe away enough of the recent past so as to reassert the authority of Congress, to reduce the power of the Presidency, and thereby to prepare to impress upon postwar public policy an unequivocally conservative stamp." He deals harshly with such Senate leaders as Republicans Robert Taft and Arthur Vandenberg and Democrats Tom Connally and Kenneth McKeller, but he agrees that to a considerable degree they were successful in shaping both domestic and foreign policies. At home the American consensus at the end of the war was much more a desire to avoid renewed depression than it was a commitment to liberal reform, and this mood helped conservatives to blunt the efforts of liberals such as Henry Wallace. In foreign affairs, Blum sees the influence of the war and long-standing national habits as enabling conservatives to limit both American political flexibility and financial generosity in the postwar period. Yet, he refuses to blame the United States for the origins of the cold war, arguing that no other major power in 1945-46 "had a broader, more generous foreign policy."

Even though he unhappily acknowledges that conservatives seemed to be winning most of the battles over policies in the mid-1940s, Blum takes pleasure in the fact that liberalism persisted: "the agenda for reform remained alive in the hearts and minds of a sprightly minority—the liberals still in Washington or at the crusading journals or eastern universities, mostly veterans of New Deal or war agencies. . . . They were bright, proud, prolific, and articulate men, at once respectful and gently jealous of each other, disinclined to accept any individual as their spokesman." Ironically, other writers have used very similar terms to describe those who later involved the United States so deeply in Vietnam.

For this reviewer the book does strike one sour note. Blum clearly reserves his real respect for eastern liberal intellectuals. Almost everyone else is handled—at best—in a vaguely patronizing tone. Blum is by no means alone in doing this, but it does become annoying. Nevertheless, this is a minor complaint when compared with the overall quality of the work. Blum is a master craftsman, and admirers of his earlier books will not be disappointed.

JIM F. HEATH  
Portland State University

GEORGE Q. FLYNN. *Roosevelt and Romanism: Catholics and American Diplomacy, 1937-1945*. (Contributions

in American History, number 47.) Westport, Conn.: Greenwood Press, 1976. Pp. xx, 268. \$13.95.

This monograph rests upon an impressive search in secular and religious sources. George Q. Flynn has established that the American Catholic Church took official positions on non-theological issues. He explains how these positions influenced twenty-one million of the faithful who were reached through the Church's vast complex of publishing media, radio programs, lay organizations, and educational institutions. Catholic relations occupied a high priority on FDR's political agenda. At times he communicated directly with leading American bishops, and in other instances he reached them by using leading Democrats of their own faith. Even the Vatican was not beyond Roosevelt's political reach, an effort which culminated in Myron C. Taylor's mission to the Holy See.

The author skillfully builds the general background to six issues treated in a topical manner. He analyzes Catholic isolationism and explores its relationship to Vatican desires. The chapter on the Spanish Civil War breaks new ground. Flynn discounts the influence of the Catholic lobby on the administration's policy of neutrality, arguing that Roosevelt's objective was to finesse the Spanish issue in order to gain greater leeway in his effort to halt direct Axis aggression.

The historic cleavage between the Catholic minority and the WASP majority is central to Flynn's story. This gap was narrowed for a time amidst a general enthusiasm for the initial New Deal reforms. After 1936, however, it was widened by Catholic support for Franco, the key Roman Catholic position in the America First coalition and the Church's unbending opposition to help for invaded Russia. With majority support assured for material aid to Britain and the USSR, the Church found itself outside of the national consensus. But after Pearl Harbor the hierarchy participated in the war effort with such enthusiasm that "Ecumenism was born in the midst of World War II."

Flynn's prose is marred by his listing in the text of too many obscure names and places. It is surprising that a scholar so familiar with the sources should repeatedly identify Sumner Welles as Assistant Secretary of State during the six years when he was Undersecretary. There is no chapter on Charles E. Coughlin whose movement is treated only in a peripheral manner. This choice is debatable, since many contemporary non-Catholics identified him with a measurable segment of grassroots Church opinion. Flynn carefully differentiates loyal Catholic support for World War II from opposition in Church quarters to the Vietnamese War, but he does not explain the erosion of Catholic authoritarianism in the last thirty years.

Hence his brilliant analysis of the tight Church structure of yesteryear will appear strange to his younger readers. Nonetheless, this book is a welcome addition to the historiography of the FDR era.

SELIG ADLER  
State University of New York,  
Buffalo

ALONZO L. HAMBY. *The Imperial Years: The United States Since 1939*. New York: Weybright and Talley. 1976. Pp. xi, 429. \$14.95.

Alonzo Hamby's *The Imperial Years* is the latest of the histories of recent America that are written from a conscious point of view for student and general readership. It is largely a history of presidential actions, although there are occasional economic, social, and cultural references, and even a well-written chapter emphasizing the social trends of the 1950s and 60s.

Hamby is avowedly a liberal democrat, and an idealistic one, as his attacks on radical and his neglect of conservative interpretations of history show. Although he is critical of presidents Franklin D. Roosevelt, Harry S. Truman, John F. Kennedy, and Lyndon B. Johnson, his views of them are rarely trenchant. Despite their failings—Roosevelt's missed opportunities, Truman's moralistic-legalistic attitude, and Kennedy's absent-minded approach to Vietnam—those liberal Democratic presidents, according to Hamby, demonstrated balance and restraint. Along with the others, even Johnson showed serious concern for a fairer distribution of wealth. Only occasionally, however, did Dwight Eisenhower and Richard Nixon match the strengths Hamby credits to recent Democratic presidents in domestic and foreign affairs. What none of them did was to reduce America's potential to unleash havoc in the world; this, Hamby indicates, was related to their inability to cope with America's weakness of always seeking complete success in any great endeavor.

Although Hamby's is a narrative history, there is plenty of interpretation in it. The overarching concept is that after 1939 the United States moved from being a simple egalitarian society toward being a "wealthy imperial nation." This rapid change of status underlies the tensions that have dominated American politics and society during the past generation as the nation has sought, often contradictorily, to extend its power and protect its world interests while yet trying to maintain its founders' ideals. One wishes this were a longer book so that Hamby could have elaborated upon that interpretation and upon the political impact of America's success orientation. They are points

worth further consideration as is his reminder that America's dwindling resources need not cause privation in such an abundantly endowed country.

One can debate much of what Hamby writes. Not only does his point of view invite it, but so do some of his historical judgments. For example, how could Roosevelt "rightly" have rejected Prince Konoye's offer of a conference of leaders in 1941? And on what sound evidence can one conclude that the Korean invasion of 1950 was "probably" approved by Josef Stalin?

All this is not to say that Hamby's book is bad. It contains a wealth of insights and basic data, and his work usually succeeds in placing events and personalities in the setting of their time instead of judging them by the present. No, the problem with *The Imperial Years* is that it is written, however intelligently, from a political standpoint. Why cannot historians stop championing *ex parte* views? Certainly, it is intellectually better to seek objectivity and to fall short than to be intentionally subjective. Hamby's latest work is proof of that.

DONALD R. MCCOY  
University of Kansas

KEITH D. MCFARLAND. *Harry H. Woodring: A Political Biography of FDR's Controversial Secretary of War*. Lawrence; University Press of Kansas. 1975. Pp. x, 346. \$12.50.

Keith D. McFarland has written the first biography of Harry H. Woodring, the ill-starred secretary of war during most of Franklin D. Roosevelt's second administration. With better than average documentation, and reasonably good prose, the author follows his subject from birth and boyhood in Elk City, Kansas, through his early career, a brief stint in the army during the First World War, and then his success in banking that left him moderately wealthy and "retired" at 42 when the Great Depression hit southeastern Kansas.

Despite the tedium of small town life in the Midwest, Woodring found excitement in American Legion politics. He converted a state commander's connections into Democratic party capital and proceeded to win the governorship of Kansas in 1930. Unique in Kansas history as a Democratic governor, Woodring used his one term to help Roosevelt win the presidential nomination and election in 1932. Woodring lost his own bid for re-election, but the new president found a position for him in Washington.

As assistant secretary of war during FDR's first term, Woodring helped the army to survive the calamitous Depression. With Secretary Dern's death in 1936, the president named Woodring acting secretary and then rewarded him with the

secretaryship for faithful political and administrative support in the election of 1936. Though he was a skilled administrator, aiming to modernize the army and improve its capability for defending the nation, the Kansan's tenure as secretary was severely marred. Woodring's inability to control Louis Johnson, his overly ambitious assistant secretary, and his unwillingness to support the president's policy of maximum armaments support to those resisting aggression in Europe, led inevitably to his replacement by Henry L. Stimson in mid-1940. Personally loyal to Roosevelt, Woodring never transcended that basic isolationism common to his home region. Attempts to re-enter Kansas or national politics failed in the postwar years. Harry Woodring became another political has-been, best remembered (if at all) as the only cabinet member dismissed by FDR.

McFarland uses eighty pages to carry Woodring through his Kansas years, including the governorship, and then spends 155 pages on the seven years in Washington. The author is good at dealing with the American Legion and with Kansas politics, though he seemed to this reviewer a bit light on his research and analysis of Woodring's gubernatorial years. His real contribution lies in describing the machinations of Louis Johnson and the budget struggles within the War Department between the air corps and the rest of the army. Harry Woodring deserved a biography, and McFarland has provided a good one.

GERALD E. WHEELER  
San José State University

NEIL A. WYNN. *The Afro-American and the Second World War*. New York: Holmes and Meier. 1976. Pp. viii, 183. \$18.50.

Two blues recordings made during the early 1940s nicely illustrate the difficulty confronting historians of the black experience in World War II (and perhaps the black experience in modern America). Josh White provided one view: "Uncle Sam says 'We live the American Way'/Let's get together and kill Jim Crow today." Arthur "Big Boy" Crudup provided the other: "I've got my questionnaire and they need me in the war/ Now if I feel like murder, won't have to break the county law." Which set of lyrics, if either, more accurately reflected the Negro's viewpoint: Josh White's optimistic, affirmative, and patriotic ones, with their explicit assumption that the war could provide a chance to eliminate racial discrimination? Or Arthur Crudup's pessimistic, cynical, and brutal ones, with their implicit recognition that the war might legitimize the release of the pent-up violence created by racial discrimination?

There is, perhaps, no entirely satisfactory way of

answering these questions. Not only can evidence be found on both sides, but the two views, which logically appear incompatible, are capable of being held by the same person at the same time. It is one of the many merits of Neil Wynn's excellent book that it recognizes the complexity of the problem. Although Wynn eventually concludes that "the militance of most Afro-Americans was directed to ensure equal participation in, not withdrawal from, the war effort," he also notes, in his discussion of cultural developments, that music and literature "reveal a certain ambivalence in blacks' attitudes, between the sometimes militant rejection of white society and the equally militant desire to be included as equal participants."

Wynn's conclusions are based on an appraisal of the war's impact on major areas of Negro life and thought. Three chapters are devoted to changes in blacks' economic status, military role, and demographic position; one to their portrayal in film, music, and literature; and one to the "psychological impact of war: black attitudes and the white response." Brief attention is also given to blacks' experiences in earlier wars and to some developments after 1949. Wynn has examined the relevant manuscript sources as well as the secondary literature. If his findings are not on the whole startlingly new, that is not necessarily a bad thing, for he provides an intelligent synthesis and does so in a clear, crisp fashion. He applies, with commendable caution, Stanislaw Andreski's theory of a "military participation ratio," which holds that government welfare policies will increase in direct proportion to the need for mass participation in a war. Wynn, even while recognizing its obvious implications for blacks during World War II, notes that the theory "is little more than common sense." That the government extended certain civil rights to blacks as wartime manpower needs escalated is, indeed, not a matter of dispute. What remains uncertain is the effect those policies had on the attitudes of Josh White's and Arthur Crudup's audience.

RICHARD POLENBERG  
Cornell University

DOUGLAS W. NELSON. *Heart Mountain: The History of an American Concentration Camp*. Madison: State Historical Society of Wisconsin, for the Department of History, University of Wisconsin. 1976. Pp. ix, 183. \$12.50.

Douglas W. Nelson's fine little monograph on a World War II Japanese-American "relocation center" provides a jarring reminder of the power of narrative to revise our historical understanding. Nelson attacks the stereotype of the docile Japanese-American patriot eagerly marching off to war



in the unswerving belief that time and his actions would finally convince white America. Though a numerical majority, this brand of patriot was not the only brand among those who were relocated. Nelson shows that governmental authorities acted with dispatch to suppress others who argued that American patriotism in this case called for protest, and pragmatic accommodationists among the Nisei obscured the record of resistance to further their prescription of assimilation through forbearance. The time has come, says Nelson (following Roger Daniels into print on this point, though the two warmly admit mutual scholarly debts), to explicate the resistance.

Nelson's narrative is simple in outline, free of moralizing, and balanced in its assessment of the motives of all parties, even bumbling Wyoming neighbors. His official, archival, and newspaper documentation is convincing, but hardly extraordinary. Nelson chooses verbs and adjectives judiciously and eschews clashing ironies. He admits the impracticality of the resistance, but insists that "the Constitution was not the principal victim of relocation; the Japanese Americans were."

There is much that Nelson does not undertake. Most notably, he never interprets Heart Mountain through the eyes of the captors (as did the contemporary study of Edward H. Spicer and associates), nor does he provide a social analysis of the "spoilage" and "salvage" of Japanese-American loyalty (as Dorothy S. Thomas and others did). What he does tell us that is new is that resistance to the ghastly American policy was coherent, purposive activity, not the tragically uncoordinated reactions of those Japanese-Americans most egregiously misclassified by government actions. Nelson shows that the resistance had leadership, grew in substantive response to threatening moves by the government, and developed an ideology. Focused on the reciprocal obligations of citizen and government, this ideology led more than a few at Heart Mountain to accept jail rather than serve in the military while their government maintained concentration camps for citizens.

*Heart Mountain* deserves an audience, for it poses freshly and unpretentiously the classic choice between resistance and pragmatic acquiescence.

JOHN MODELL  
*University of Minnesota,  
Twin Cities*

EARL F. ZIEMKE. *The U.S. Army in the Occupation of Germany, 1944-1946*. (Army Historical Series.) Washington, D.C.: Center of Military History. 1975. Pp. xvi, 477. \$9.00.

With the publication of this book the seventy-nine-volume history of the Army in the Second World

War, planned and published by the Center of Military History, nears completion. Only seven volumes, including the master index, remain to be published in this highly regarded series. Designed primarily for the education of professional soldiers, most of the volumes hitherto published have treated such matters as strategy, tactics, organization, logistics, and command decisions. This volume which provides an account of the U.S. Army in the military government and the occupation of Germany until mid-1946, will certainly be of value to professional soldiers but it will have equal, and perhaps even greater, interest for scholars and the general reader.

The records relating to World War II are truly awesome both in variety and volume. Ziemke has mastered the documentation to a greater degree than any previous writer on the subject and he deftly guides the reader through the highlights of planning, training, and operations at all levels from Washington and theater headquarters down to the military detachment in the field. His organization is primarily chronological following the general outline suggested by the development of the combat story. This method has the advantage of conveying to the reader a sense of the immediacy of unfolding events, but it also results in a certain disjointedness. Perhaps in a book dealing with so many difficult and discrete subjects it might have been better to adopt a topical approach, so that the reader could find in one place a connected account of such topics as concentration camps, denazification, refugees and displaced persons, and the efforts of the Army to preserve monuments, fine arts, and archives from unnecessary destruction. But this flaw, if it is one, does not detract from the fact that this is a major contribution to the history of World War II and of modern Germany. Not the least of the virtues of this book is to suggest both implicitly and explicitly numerous topics for further research not only by historians but by political scientists, economists, and sociologists.

HARRY L. COLES  
*Ohio State University*

LEONARD MOSLEY. *Lindbergh: A Biography*. Garden City, N.Y.: Doubleday and Company. 1976. Pp. xxx, 446. \$12.95.

This general study, written in the same popular vein as earlier accounts by Kenneth Davis and Walter Ross, is the first biography to appear since the aviator's death in 1974. The author, a well-known British journalist and novelist who lists *Hirohito* and *The Reich Marshal* among his recent publications, touches on most aspects of Lindbergh's varied life. Included are discussions of his Minnesota background, barnstorming, air mail



days, the 1927 flight, family life, the kidnaping incident, his European contacts, especially in England and Germany, his America First and World War II activities including the Roosevelt administration's cabinet "offer" in 1939, and Lindbergh's later conservation efforts. Mosley devotes almost one-third of the volume to the decade between 1935 and 1945.

The book is consistently journalistic in approach, style, and judgment. While it is a lively and readable account, its authenticity is weakened by many inaccuracies of detail (e.g. "350 to 400 gallons" [the *Spirit of St. Louis* carried 450], p. 84; "Democratic senator George Norris," p. 180; and Charles Augustus [August] Lindbergh, Sr.," p. 6), and, although there is a source note section, there is no precise documentation or general footnoting. Moreover, the author's choice of words is often questionable (e.g., "progressist ideas," p. 14; "he hadn't peed," p. 108; "she [Mrs. Lindbergh] wanted to breed," p. 185; "Peacenik," p. 247; and "quiverful" [of children], p. 368), and there is a tendency to expand narrative dialogue.

*Lindbergh: A Biography*, at times sympathetic and at times critical of its subject, does incorporate sources such as Wayne Cole's *Charles Lindbergh and the Battle against American Intervention*, Lindbergh's own *Wartime Journals* and *Boyhood* commentary, the Roosevelt Papers, the letters of Harold Nicolson, and several unidentified interviews. But numerous other manuscript collections, newspapers, and documents have not been consulted. The book is weak on Lindbergh's role in aviation history since 1927. Some insights emerge, however, about Lindbergh the man. For example, his transition from an expert scientific technician to an ardent ecologist genuinely concerned that civilization could become the victim of its own technology is clearly noted. The book provides a general overview of Lindbergh's life for the lay reader, but it is not a definitive scholarly work.

BRUCE L. LARSON  
Mankato State University

JOHN BARTLOW MARTIN. *Adlai Stevenson of Illinois: The Life of Adlai E. Stevenson*. Garden City, N.Y.: Doubleday and Company. 1976. Pp. ix, 828. \$15.00.

For readers who joined this reviewer in casting their first presidential ballot in 1952 for Adlai Stevenson, this biography will be intriguing, while shattering some carefully preserved illusions. The subject comes through vividly, warts and all. The result is a richly detailed portrait of one who was charming, witty, gregarious, capable, stingy, self-deprecatory, egocentric, and enjoyed a mutually attractive relationship with beautiful women.

Stevenson's youthful experience included moderate familial wealth and prestige, incompatible

parents, and the unfortunate accidental experience of killing a young girl when he was thirteen. The latter, according to the author, "may have served the necessary unconscious function of turning triumphs into disasters, honors into burdens, as unconscious penance for his deed" (p. 44). Academically, he made "gentlemanly Cs" at Princeton, flunked out of Harvard Law School, and received a law degree from Northwestern University. He practiced law in Chicago and rendered the expected amount of public service, becoming particularly interested in foreign affairs and the United Nations. He belatedly entered political life, winning the Illinois gubernatorial race in 1948 (the year his wife divorced him), and was a moderately successful governor. Four years later, his was a relatively fresh face to scandal-ridden Democrats looking for a presidential candidate. Stevenson wanted the presidency, but knew that he must not appear as the Truman administration candidate. His strategy of reluctance succeeded, and the Democratic convention drafted him to run against the very popular General Eisenhower. Interestingly, both candidates were basically liberal in foreign policy and conservative in domestic affairs. Yet liberals, fearful of a military man in the White House, were attracted by Stevenson's imaginative rhetoric and subsequently adopted him as one of their saints.

The author, a Stevenson speech writer in 1952, has successfully mined the Stevenson papers and memories of contemporaries. Although a closer examination of secondary works might have provided a better analysis of such problems as Eisenhower's difficulty with McCarthyism and the results of the 1952 election, this is a very solid, candid portrait of Stevenson's life through the 1952 campaign. Those interested in the American post-war period will profit by reading this biography. One hopes the author will continue the study in a future volume because, as he concludes, "the best years of Adlai Stevenson's leadership lay ahead [of 1952]." In that case, he will have to make the definitive judgment of Stevenson which he avoids here.

R. ALTON LEE  
University of South Dakota

WILLIAM HOWARD MOORE. *The Kefauver Committee and the Politics of Crime, 1950-1952*. Columbia: University of Missouri Press. 1974. Pp. xii, 269. \$12.00

*The Kefauver Committee and the Politics of Crime* is an impressively researched and original contribution to the study of recent American history. William Howard Moore's major theses are provocative and well argued. His sketches of Kefauver and other key figures in the investigation are tersely informative; and he displays a sure grasp of congressional

politics, personalities, and procedures. His essay on sources is an excellent guide to the literature.

Like the Kefauver Committee itself, Moore focuses mainly on gambling and on the connections between gamblers and urban politicians. His principal thesis concerns the Kefauver Committee's understanding of organized crime and how, through the committee's investigation, this understanding came to be so widely popularized. According to Moore, crime in America has always been a relatively decentralized enterprise, characterized mainly by city-wide and regional agreements. The concept of the Mafia as a highly centralized, "alien, corrupting, atavistic, conspiratorial group," he writes, is largely myth, created in response to the rise of urban-industrial culture and popularized in the twentieth century by journalists, citizen crime commissions, and the Federal Bureau of Narcotics. The Kefauver Committee, whose televised hearings attracted widespread public attention in the spring of 1951, dramatically reinforced and spread this concept. "Largely ignoring the economic, legal, and social conditions giving rise to organized crime, the Committee implied that it essentially originated outside of American society and was imposed upon the public by a group of immoral men, bound together by a mysterious ethnic conspiracy." The consequences of the Committee's investigation, concludes Moore, included the mis-education of the American public and the misdirection of local, state, and federal approaches to crime for more than two decades.

Estes Kefauver emerges from this study as an ambitious though occasionally inept freshman senator who sought to exploit the crime issue for personal gain, but who also tried, unsuccessfully as it turned out, to prevent the investigation from embarrassing Truman and the Democrats. The nation-wide publicity attracted by the committee, however, and especially by the televised hearings in New York City, cost Kefauver support within the party even as it inflated his public image. He suffered "heavily and probably unjustly in the eyes of his own party, but he . . . benefited immensely, also probably unjustly, in the eyes of the press, who came to depict him as something of a Saint George fearlessly slaying the dragon of crime and corruption." The hearings thus made Kefauver a national celebrity and presidential contender, but also aroused the antagonism of urban regulars who would ultimately deny him the Democratic nomination.

Moore's study is not entirely free of faults. His contention that crime in America has been decentralized and local in orientation is plausible and probably accurate, but needs to be more fully documented. So does his tantalizing comparison of

the politics of crime and the politics of anticommunism. Although Moore's writing is clear and generally attractive, he has a tendency to lapse into the vernacular of the criminal justice subculture with frequent references to "muscle," "protection," "lay off operations," and so forth. These criticisms aside, Moore has produced an important and richly informative study of politics and society in the recent period.

ROBERT GRIFFITH  
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Amherst

JAMES W. ELY, JR. *The Crisis of Conservative Virginia: The Byrd Organization and the Politics of Massive Resistance.* (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1976. Pp. vii, 220. \$13.50.

James W. Ely, Jr. assesses the dilemmas that plagued Virginia traditionalists during the 1950s and early 1960s, when a combination of federal court orders and black community initiative threatened white control of public policies. An entrenched political organization headed by Harry F. Byrd, Sr., a conservative press corps led by James J. Kilpatrick of the Richmond *News Leader*, and bitter-end resisters in and out of public office charted a defiant course that ultimately failed to sustain states' rights and white supremacy. As Ely shows, however, advocates of massive resistance did manage to jeopardize the state's educational system, to mislead the public into imagining that segregation could be maintained under law, and to create some serious though not fatal divisions within the Byrd machine itself.

The author depicts the machine's background, the ineffectual political forces periodically arrayed against it, the ineptness of Virginia moderates, the legal and procedural tactics employed to forestall integration, and the transition from massive to passive resistance after 1958. And yet he has failed to capture the human and public tragedies in all this. With its heavy reliance on official records, prepared statements, citations from leading newspapers, and manuscript collections of the state's leading personalities, the study is basically a traditional political history. Ely did interview such prominent figures as Harry F. Byrd, Jr., editors Virginius Dabney of the Richmond *Times-Dispatch* and James J. Kilpatrick, congressman Howard W. Smith, governors J. Lindsay Almond, Jr., and Mills E. Godwin, Jr., among others, but discussions of the principals and of political infighting too often seem underdeveloped.

Perhaps the real problems were conceptual and methodological. Beyond references from the Rich-

mond *Afro-American* and the *Norfolk Journal and Guide*, Ely disregarded most black sources. Interviews with black Virginians and citations from the NAACP papers at the Library of Congress fail to appear. Although the author warns us that his concerns are with Virginia's elites (p. vi), that provides a far too narrow focus for analyzing desegregation questions that continue, a quarter century after *Brown* and its companion cases, to command such widespread national attention.

ROBERT L. ZANGRANDO  
*University of Akron*

H. CARLETON MARLOW and HARRISON M. DAVIS. *The American Search for Woman*. Santa Barbara, Calif.: Clio Books. 1976. Pp. xiv, 539.

Since 1970 the field of women's history has produced important scholarship and has reached the point where historians are becoming aware that old conceptions and methodologies do not suffice. The scholarship of Gerda Lerner, Carl Degler, Linda Kerber, Peter G. Filene, Joan Kelly-Gadol, and others challenges certain assumptions that historians make. Lerner offers a particular challenge: "While most historians are aware of the fact that their findings are not value-free and they are trained to check their biases by a variety of methods, they are as yet quite unaware of their own sexist bias and, more important, of the sexist bias which pervades the value system, the culture, and the very language within which they work" ("Placing Women in History: A 1975 Perspective," [p. 362] in Berenice A. Carroll, ed., *Liberating Women's History* [1976]).

Unfortunately, H. Carleton Marlow and Harrison M. Davis seem unaware of this challenge, or of exciting developments in this emerging field of history. The work is not as it claims, an "unemotional examination" of the women's movement, the role of women, and their sexual nature, placed within "a needed historical perspective" in order to "diffuse the explosive atmosphere" associated with the Equal Rights Amendment. The authors omit recent works, place the women's movement within traditional historical delineations, and base their study on a patriarchal value system. They present four conflicting "pillar ideas": 1.) "inatism"—that women inherit an inferior sexual nature; 2.) "environmental feminism"—that men and women are equal; 3.) "superior feminism"—that women are superior; and 4.) "differential equality"—that biological differences between the sexes render women unsuited for equal rights and responsibilities.

Marlow and Davis favor differential equality which "appeals for recognition of biological and

structural sex differences that give each sex different work abilities" (p. 30). Their central issue in the ERA debate is whether there are "significant physiological, psychological, and character differences between the sexes." If sex differences exist, "then passage of the ERA may be detrimental to the majority of women and men" and will "promote unequal and undesirable prejudicial treatment of the average woman and man" (p. 414). These two authors contend that separate is equal. This is a blatantly partisan book offered under the guise of objectivity.

MOLLIE CAMP DAVIS  
*Queens College,  
Charlotte, North Carolina*

ELMO R. ZUMWALT, JR. *On Watch: A Memoir*. New York: Quadrangle. 1976. Pp. xv, 368. \$12.50.

This memoir by a retired chief of naval operations (1970-74) has considerable value for the American historian who wishes to make his lectures on the Nixon administration more than just a recitation of events. Except for a few pages of autobiographical background, Elmo R. Zumwalt rigidly restricts his account to those years when he "had the watch." Because of his position, Zumwalt's concerns were those which involved naval management, operations, and the navy's role in the national security structure. Unlike most military figures, the admiral writes fully and candidly about the people with whom he worked. He praises the worthy and delineates sharply the faults of those he found lacking. Three national figures are keelhaunched in the course of his somewhat remarkable account. The admiral judges Secretary of State Henry Kissinger as the most ignoble of the president's inner circle for sacrificing the nation's nuclear defenses to seek détente with the Soviet Union. Admiral Hyman Rickover, who epitomizes the national commitment to nuclear defense by the submarine-launched ballistic missile system, is found wrongheaded at every turn. His "high cost" nuclear submarines and aircraft carriers rob the navy of sufficient "low cost" surface vessels to match the growing strength of the Red Fleet. Zumwalt does not believe the Russians seek nuclear conflict, but he does believe they will dominate the oceans and the maritime states unless America can match their power.

When not blazing away at Kissinger or Rickover, the admiral excoriates General Alexander Haig, President Nixon's military adviser and confidant during the last scene of the *Götterdämmerung*. Driven by unbounded ambition, the general and the secretary of state blocked any attempts of the

admiral to convince President Nixon that the Strategic Arms Limitation Talks were a sham. Very obviously, Admiral Zumwalt's views are not the final word on affairs within the Nixon administration, but they do provide an interesting contrast to those who have written admiringly of Kissinger's diplomacy and Rickover's sagacity. For aficionados of modern naval affairs, these memoirs are a splendid mine of information on flag-officer selection and assignment, budget practices, fleet operations, and the folkways of the Joint Chiefs of Staff.

Possibly the most interesting part of the book lies in the series of chapters concerning Zumwalt's attempts to modernize the navy's personnel practices. The "Z-grams" (policy directives) dealing with dress, hair style, race relations, sex discrimination, abrasive regulations, and 115 other topics were the admiral's means of making the navy more reflective of the society it serves. Putting first things first, Zumwalt knew that unless he made the navy a more humane institution he would not be able to attract and keep the men and women necessary to operate his fleet. Admiral Zumwalt did change the navy; but the permanence and value of his work is yet to be determined.

GERALD E. WHEELER  
*San José State University*

WELDON A. BROWN. *The Last Chopper: The Denouement of the American Role in Vietnam, 1963-1975*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1976. Pp. x, 371. \$15.00.

Part scrapbook and part postmortem, this well-intentioned and well-printed volume attempts to illuminate America's Vietnam experience by recapitulating passages of the national dialogue on Southeast Asia as it developed under the Johnson, Nixon, and Ford administrations. (The author dealt in an earlier volume with the origins of the Vietnam problem in 1940-63.) Having made his patient way through a text of some 200,000 words, this reviewer cannot in conscience encourage others to follow his trail. Readers without a firm preliminary grasp of the Vietnam story must inevitably be baffled by the author's assumption of prior knowledge as well as his repetitiveness and apparent hospitality to conflicting viewpoints. Those already knowledgeable about Vietnam, on the other hand, can hardly gain much fresh enlightenment from a rehearsal of familiar arguments drawn from sources that range from the *Pentagon Papers* to the *Roanoke Times*.

Such merit as can honestly be claimed for *The Last Chopper* arises mainly from Weldon A. Brown's evident determination to be fair to every segment of American opinion, even at the cost of slighting the articulation of his own views. "Within

a few decades," he writes in a characteristic passage, "it will not be easy, if at all possible, for our children to appreciate this war and the objectives of those very sincere men who decided it was necessary. Equally important is it that we should understand those dedicated spirits who denounced our intervention in this war, resisted the draft, and sought finally with success our withdrawal." One wishes the author's labor gave promise of contributing more substantially to this desirable result.

RICHARD P. STEBBINS  
*Council on Foreign Relations*

## CANADA

CORNELIUS J. JAENEN. *Friend and Foe: Aspects of French-Amerindian Cultural Contact in the Sixteenth and Seventeenth Centuries*. New York: Columbia University Press. 1976. Pp. 207. \$12.50.

A distinguished panel of historians selected Cornelius J. Jaenen's study as the winner of the 1973 Sainte-Marie Prize given by the government of Ontario for "original research and contribution to the knowledge and understanding of seventeenth century Canada." It brings together a considerable body of detailed information, arranged topically, dealing with the history of contact between the French and the Indians in Canada. By examining the contact between these groups in terms of their respective cultures as well as the circumstances of their interactions, Jaenen has provided a valuable revision of an important aspect of early Canadian history.

One of the first stereotypes to fall in this re-examination is Parkman's characterization of Spanish, English, and French behavior and its effects on the native people. Jaenen finds an undercurrent of discrimination and prejudice, although it was not given official or legal status.

His topics include the activities and attitudes of the missionaries and the native response, and the impact of the Indian culture on the newcomers, which raised questions of cultural relativism. French understanding of native culture—including religion, political organization, child-rearing, and alcoholic consumption—are examined. The "noble" and the "ignoble" "savage" as seen in France and New France were part of the impact of the New World on the Old. The impact of the French on the Indians was much harder. Cultural change was taking place by nondirected and directed means. This meant disruption and destruction for individuals and groups.

At one level of generalization common experiences can be found in many of the contact situations. In another perspective it is clear that the



contact experiences of particular peoples, "tribes," bands, or other units were unique. In a study that deals with such close detail, the latter perspective might have been more clearly delineated.

Both Canadian and non-Canadian historians and social scientists have contributed to historical writing at both of these levels in the past several decades. Jaenen's book is an important addition to this body of studies which attempts, aided by the sources and perspectives of history, ethnohistory, anthropology, archeology, and other social sciences, to reconstruct a new history of Indians and of Indian-white relations.

E. PALMER PATTERSON  
*University of Waterloo*

G. P. V. AKRIGG and HELEN B. AKRIGG. *British Columbia Chronicle, 1778-1846: Adventurers by Sea and Land*. Vancouver: Discovery Press. 1975. Pp. xv, 429. \$14.95.

The authors of this book have undertaken to produce a multi-volume history of Canada's Pacific Coast province. This, the first of the series, takes the story from the days of Captain James Cook to 1846, when Great Britain surrendered the southern Columbia to a bellicose United States. The book closes with a brief lament "for our lost kingdom . . . a goodly, goodly land. Now forever lost." The second volume will deal with the years from 1846 to 1871 when British Columbia entered the Canadian Confederation.

The extent of the authors' reading is considerable. It obviously ranges over the whole field of early British Columbia history, including early explorers such as Drake, Cook, Quadra, Vancouver, Mackenzie, Fraser, Thompson, Simpson, Ogden and Black; the early traders and their uneasy relations with the Indians; the rivalries of the fur companies—American, British, and Canadian; the crisis posed by American immigration to the lower Columbia valley; the questionable role of that "ambivalent mammoth," John McLoughlin; and the American takeover in 1846. This is all very interesting, but have not previous writers on British Columbia, from the late Judge Howay to Margaret Ormsby, done the same thing?

Let it be understood that the authors are historians of British Columbia by affection rather than by métier. Certainly the strength of their affection is apparent to anyone who takes the time to read this book. The critical reader may, however, consider the result none too satisfactory; he may, indeed, say that the book suffers from the uncritical love of the authors for their subject. G. P. V. and Helen Akrigg state that they have worked in over thirty libraries and archives in Canada, the United States, Great Britain, Australia, and New Zea-

land, in their quest for original materials. The references, however, suggest a greater familiarity on the part of the authors with the printed than with the manuscript sources.

The most original aspect of this book is its approach. The authors call their work a "chronicle" and that is precisely what it is—an account by years and not by topical chapters. Sometimes a year may yield little more than a short paragraph of text, sometimes several pages. This approach may be useful for reference purposes (if the reader is concerned with chronology), but the overall effect is of jerkiness and lack of coherence which will annoy some readers. It did annoy me. The prose, however, is simple and straightforward, and the reader will probably take pleasure from the numerous little episodes which give life and vitality to history, even if they do not contribute a great deal to an understanding of it. The story is spiced with violence, sex, and even a few Yankee spies. There are also a number of good illustrations.

If the reader is looking for entertainment, say a little bedtime reading, this is his book. Otherwise he will probably prefer to reread Margaret Ormsby's *British Columbia: A History*.

GEORGE F. G. STANLEY  
*Mount Allison University,  
New Brunswick*

MICHAEL B. KATZ. *The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City*. (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1975. Pp. xiii, 381. \$17.50.

There will be few in the field of North American urban history who will not already be familiar with Michael Katz's work even though this is the first book to come from the collaborative quantitative project he directs. In fact, the evolution of this volume has been a sort of public affair. Five large interim reports, several articles, and innumerable references and comments by other scholars precede the book. The author regards *The People of Hamilton* as a report on an ongoing project and plans another volume incorporating essays by different project members. One such essay, an excellent analysis of the social geography of Hamilton by Ian Davey and Michael Doucet, is included as an appendix to the present work. An outside observer can only be impressed with what Katz and his colleagues have accomplished and especially the extent to which concepts have been refined and perspectives broadened since the publication of the first article in Thernstrom and Sennett's, *Nineteenth Century Cities* in 1969. The result is a major achievement in the general field of urban



history and a breakthrough in the historical study of class in Canada.

At the most basic level Katz carefully reconstructs the social and family patterns within Hamilton, Ontario, concentrating on the decade of the 1850s. His general conclusion is that the two underlying themes of nineteenth-century urban history were inequality and transiency. He demonstrates the distance between the rich and the poor in several ways and suggests that the degree of inequality increased rather than decreased with the onset of industrialization. Transiency was found to be as characteristic of Hamilton as several recent studies have shown it to be for American cities. Throughout this study Katz is concerned with the question of the extent to which his findings for Hamilton represent patterns common to other mid-nineteenth-century cities. His chapters abound with American and British comparisons, but unfortunately include only a few references to the Canadian urban experience. He is correct in stating that there is "an absence of comparable works on Canadian cities," but his conclusion that Canadian comparisons are therefore a "currently insurmountable problem" is more of a commentary on his research interests than an accurate description of the present state of Canadian urban and social history. The footnotes indicate his wide reading in the literature of social theory and quantitative studies elsewhere, but virtually none of the relevant Canadian literature is cited, except that written by his own students.

At another level, Katz has broader objectives. He hopes to reformulate some of the big questions about social change because he believes that a good deal of social theory is based on assumptions which are historically inaccurate. One such question is the identification and explanation of the social effects of industrialization as part of a process of modernization. Much of the present discussion of the subject, he argues, falsely contrasts traditional rural society and industrial urban society, whereas the industrial city represents a transformation of the preceding stage, the commercial city. He makes a compelling case for the hypothesis that the society he is examining in detail in Hamilton is representative of a social system which persisted for several centuries in the Atlantic world and constituted an urban type distinct from the industrial city. Thus, he suggests that the commercial city of Hamilton in the 1850s was more like the commercial city of Salem, Massachusetts in 1800 than it was like the industrial city Hamilton became in the 1880s.

In some respects the author overwhelms the reader with his presentation of masses of detail and his speculation in the field of social theory. Probably only initiated devotees of the quan-

titative approach will actually enjoy some of the incredibly complex sections. And yet, the compensations far outweigh the difficulties. Katz has proven himself a master not only in developing new methodologies, but in asking new questions and demolishing old myths. His insights and hard data will provide a sound basis for future comparative works.

GILBERT A. STELTER  
*University of Guelph*

## LATIN AMERICA

LESLIE B. ROUT, JR. *The African Experience in Spanish America: 1502 to the Present Day*. (Cambridge Latin American Studies, number 23.) New York: Cambridge University Press. 1976. Pp. xv, 404. Cloth \$23.95, paper \$6.95.

Leslie B. Rout, Jr. has done what most of us would have feared to contemplate. He has written a history of the African experience in Spanish America from Columbus to the present. Not content to concentrate on slavery, Rout has attempted to encompass the total experience of blacks in the Spanish American world. No small task, that. Remarkably, the result of this effort is a wide-ranging, thoroughly researched, and stimulating account of what happened to the Africans who were brought to Spanish America. Fully aware of the gaps in our knowledge, the inconsistencies in the data, and the controversial interpretations of scholars, Rout provides us with the most complete and satisfying account of Spanish American black history available.

To be sure, any book that attempts to cover the slave trade, the practice and theory of slavery, slave rebellions and resistance, free blacks, slaves and blacks in Spanish American independence, and the status of blacks in republican Spanish America must of necessity fail to satisfy all critics. But most of the failings of this book are reflections of the failings in the monographic literature on the black experience in Spanish America. Where Rout missteps, it is because his authorities erred. Where his interpretations ring false, it is usually because the work he has used as a model is deficient.

Leslie Rout has provided us with a coherent, careful, and comprehensive history of blacks in Spanish America. As an American black scholar, he has endowed his survey with a special point of view developed through considerable travel and research in Spanish America. The result is more moralizing than may be necessary, a touch of naive outrage, but also an immediacy and personality that give the book its special charm.

Those of us teaching courses on black history in the Americas must thank Rout for a most valuable

classroom text, and those of us working on the history of blacks in America will welcome an account that illustrates so neatly what we know and what we do not.

JOHN V. LOMBARDI  
Indiana University,  
Bloomington

ERNEST A. DUFF and JOHN F. MCCAMANT. *Violence and Repression in Latin America: A Quantitative and Historical Analysis*. With WALTRAUD Q. MORALES. New York: Free Press. 1976. Pp. xii, 322, \$15.95.

This book represents a bold and ambitious attempt to uncover the social determinants of the political uses of force in contemporary Latin America. In their approach to the subject, Ernest A. Duff and John F. McCamant make a sharp distinction between violence, which they define as rebellious action by the people against the government, and repression, which they define as coercive action by the government against the people. By studying variations in the level and frequency of these phenomena among the countries of Latin America, mainly in the 1950s and 60s, the authors search for the sources of "social cohesion," which they define as the relative absence of both violence and repression.

Part 1 presents the quantitative analysis. Here it is immediately apparent that, in contrast to most historians, Duff and McCamant have made no effort to work with primary source materials. Instead, they have taken their information on armed movements, states of seige, press restrictions, income levels, government spending, and other such items for the countries of postwar Latin America from readily obtainable secondary sources, including international yearbooks—a procedure that raises some questions about the quality and reliability of the data. But they have brought a great deal of material together in a useful way, making the year-by-year compilations available to all their readers. They have confronted formidable problems of measurement openly, honestly, and creatively. And, with considerable skill and sophistication, they have subjected the data to rigorous statistical analysis. From multiple regression emerges their central conclusion: "Social cohesion . . . is high where institutionalization of the society's structures, particularly the military, is high, where educational expenditures are high, and where economic growth outpaces social mobilization" (p. 131).

In part 2 the authors try to relate this finding to case histories of individual Latin American nations from independence to the present. Such a shift in focus—from a cross-sectional to a longitudinal

perspective—is extremely rare in modern social science, and Duff and McCamant are to be commended for their courage in attempting it. But the result is disappointing. The case studies are superficial (some being only one page in length), impressionistic, and weakly integrated with the preceding statistical section. Part 3 consists of a single chapter on comparative history, and it is only slightly more successful than part 2.

Despite its drawbacks, this book has substantial merit. The topic is important, the conceptualization is clear, the methodology is challenging (if debatable), and the prose is crisp and lucid. This may be poor history, but it is good political science, and historians have much to learn from it.

PETER H. SMITH  
University of Wisconsin,  
Madison

COLIN A. PALMER. *Slaves of the White God: Blacks in Mexico, 1570-1650*. Cambridge, Mass.: Harvard University Press. 1976. Pp. viii, 234. \$13.50.

Until the middle of the seventeenth century, Mexico was the single most important destination for African slaves in Spanish America. Well over 100,000 Africans reached its shores and were absorbed into the dominant Indian/white population structure. The resulting amalgamation of races and the coexistence and eventual decline of African slavery as well as Indian forms of servitude and serfdom have fascinated scholars interested in the comparative experience of African slaves within the various New World cultures. It has even been argued (e.g. by Pierre Van Den Berghe) that Mexico was unique in the New World because of the total absorption and integration of a large black population into a heterogeneous mestizo nation.

Given these circumstances, it is surprising that the blacks of Mexico have received so little attention. Aside from a few short studies of particular aspects of the black experience, the only major work to date has been the study by the Mexican anthropologist Gonzalo Aguirre Beltrán. Thus the decision of Colin Palmer to begin an analysis of the most formative and important years of Mexican African slavery is an important one. With so little known, and so vast an area to be studied, he primarily conceived of his task as filling in the broad outlines, rather than attempting a detailed local and/or economic sector study. In this aim he has succeeded admirably; his work provides the best single introduction to the study of the Afro-Mexican experience.

The work begins with a survey of the slave trade and of the economic, political, and social role of

the slave in Mexican society. Palmer then defines the patterns of resistance, of slave religious and magical practices, and ends with a discussion of manumission.

Palmer's detailed analysis of the *asiento* documents in the Spanish archives is an original contribution and considerably modifies upward Curtin's estimates of forced black immigration into Mexico and Spanish America. His assessment of the "treatment" of slaves within the society is both balanced and perceptive and deals with the very important subset of relationships that existed between blacks and Indians. His rapid survey of working conditions provides a good overall view of the agricultural, mining, and industrial activities of the slaves. But above all, he has mined the records of the Inquisition for slave beliefs and has obtained some fascinating material on African survivals as well as Indian and Spanish acculturations. In all of this well-balanced work, Palmer indicates full awareness of the current debates in the study of slavery and the Afro-American experience. *Slaves of the White God* is thus a much needed addition to the literature, and along with the work of Frederick P. Bowser on sixteenth- and seventeenth-century Peru, opens up a whole new area of black studies in the Americas.

HERBERT S. KLEIN  
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JAN BAZANT. *Los bienes de la Iglesia en México (1856-1875): Aspectos económicos y sociales de la Revolución liberal*. (Centro de Estudios Históricos; New series, number 13). Mexico City: El Colegio de Mexico. 1971. Pp. xiii, 364. \$5.50.

This detailed, technical study, an English version of which was published almost simultaneously (*Alienation of Church Wealth in Mexico. Social and Economic Aspects of the Liberal Revolution, 1856-1875*, edited and translated by Michael P. Costeloe [1971]), examines the disentailment and nationalization of ecclesiastical property in Mexico and analyzes the effects of the process, particularly in the Federal District and the states of Puebla, Veracruz, San Luis Potosí, Michoacán, and Jalisco. The primary focus is on the critical 1856-67 period. The areas studied contained about two-thirds of total ecclesiastical wealth, they were affected differently by the 1858-60 civil war, and they provide a representative cross-section of economic activities. Jan Bazant is thus able to compare the liberal attack on Church property and the results in different political and economic contexts. He shows that Mexican merchants, public officials, and *hacendados* purchased the bulk of the property, and he explains changes that occurred between the time of the initial sales in 1856-57 and the early 1860s

when, for example, foreigners, especially Frenchmen, figured importantly. He concludes that over a thirty-year period or so financial gains were small for most purchasers.

The alienation of Church wealth took place during years of turmoil which greatly affected the disposal of corporate property and helped to frustrate liberal goals to carry out an agrarian reform and a public works program and to achieve a healthy financial system. These failures, in turn, had important implications for Mexico's future.

Bazant's research is impressive. He has unraveled a confused and complex aspect of a critical period, explaining with considerable clarity the complicated process and the results of disentailment and nationalization. He has also provided liberal doses of statistics and specific cases of various kinds of property transactions.

Bazant's selectivity in regions studied and in notarial records examined, the emphasis on the wealthy and purchasers of large amounts of property, despite the logical reasons for doing so, could obscure differences in implementation of the laws elsewhere, effects for the masses of society, and even some aspects of the reform in the regions studied. But, while one might be mildly disappointed over what he has not done and might question some points of analysis and conclusion, Bazant has filled an important gap in our knowledge and understanding of the Mexican Reform era.

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Stevens Point

K. E. INGRAM. *Manuscripts Relating to Commonwealth Caribbean Countries in United States and Canadian Repositories*. St. Lawrence, Barbados: Caribbean Universities Press, in association with the Bowker Publishing Company. 1975. Pp. xxix, 422.

The collections of family, estate, and mercantile papers in foreign repositories provide a rich source for the social and economic history of the Caribbean region, where little documentation of this nature has survived natural hazards and human indifference. While the largest concentrations of family and business papers may be in the metropolitan countries, significant holdings exist in the United States and Canada as a result of centuries-old family and trading ties and the acquisition policies of American libraries. This compilation is the first extensive survey of such material in North America relating to the Commonwealth Caribbean.

The survey contains 1040 entries, varying from single manuscripts to collections comprising hun-

dreds of items. The detailed and clearly presented entry descriptions include subject content and personal names mentioned. Additional access is provided by a chronological short-list of entries and an index; this reviewer checked random references of this detailed index and confirmed its accuracy. There is a list of works consulted, and the author's informative preface notes other guides to sources inside and outside the region; of the International Council on Archives' proposed guides he mentions, the British one (*A Guide to Manuscript Sources for the History of Latin America and the Caribbean in the British Isles*, Peter Walne, ed.) appeared in 1973.

K. E. Ingram's admirable work fills an important need, and the information so carefully assembled should prove invaluable to researchers in the field of Caribbean historical studies.

ANNE CAIGER  
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MAVIS CHRISTINE CAMPBELL. *The Dynamics of Change in a Slave Society: A Sociopolitical History of the Free Coloreds of Jamaica, 1800-1865*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1976. Pp. 393. \$18.75.

The nineteenth century proved to be as critical a turning point in the history of Jamaica as it was in the history of the Western world. The slow disintegration of the South Atlantic System left the former sugar-producing colonies bereft of their importance and confronting severe internal social and political problems. Everywhere challengers arose to force the defenders of the old order into reluctant retreat. In Jamaica a multifaceted revolution overtook the decaying and decadent plantation structure of sugar and slavery. The most dramatic aspect of this revolution was the general manumission of slaves between 1834 and 1838, but the most portentous action turned out to be the enfranchisement of the intermediate free nonwhite community and their participation in the political process.

*The Dynamics of Change in a Slave Society* meticulously traces the complex process of this political evolution in a colonial society beset by the concomitant collapse of both its labor system and its basic economy. Based primarily on the extensive official correspondence of the Public Record Office, this book is an extremely important and highly original work written by a woman sensitively involved in her subject. Mavis Campbell offers a general view of economic and political conditions of Jamaica at the beginning of the nineteenth century, not only within the British Empire, but also within the Caribbean and Atlantic trad-

ing and political systems. The focus of her study is the struggle for social and political acceptance of the free colored sector of the Jamaican population. This struggle began with the unique Jamaican law of 1733 which designated any individual with above fifteen-sixteenths white blood as legally white (provided such individual "be baptized"), as well as all descendants. Unlike historians Carl Degler and Winthrop Jordan, Campbell sees the law less as a manifestation of a liberal racial attitude on the part of the Jamaican planters than as an attempt to augment a "dangerously small" white population. The study examines the private acts which prior to 1813 enfranchised a selected number of wealthy, cultured nonwhites and the acquiescence in the general request by mulattos for the franchise in 1830. This latter action Campbell sees as indicative of the fear and distrust of planters for the liberal government in Great Britain and of their confidence that the mulattos would oppose the emancipation of the slaves. Not only did emancipation arrive, however, but the recently enfranchised free coloreds formed an alliance with the urban Jews and, with the connivance of some colonial governors, formed a political party which obstructed the traditional operation of island politics and eventually set the stage for the notorious demise of 1865.

This is a challenging work which frequently bristles with severe indictments of the free coloreds generally and individually. Campbell asserts controversially that "if the society of Jamaica today displays traces of complexional prejudice, as many have attested, then it is more the result of the behavior of the mulattos than of the whites. Admittedly, the whites introduced racial prejudice into the island, but the mulattos perpetrated complexional discrimination with feverish vigor" (p. 193). Allowances are made for Richard Hill and George William Gordon, but the harshest judgments are passed on Edward Jordan.

A number of debatable statements and unconvincing conclusions are offset by some particularly shrewd insights into the sociopolitical structure of Jamaica. She explains the preponderance of the free coloreds in the cities, the subtle political association of property assessment and enfranchisement, the difficult position of colonial administrators, and the affinity of free blacks for the occupation of elementary school teaching. In sum, this book represents a substantial contribution to the field, admirably complementing the works of Philip Curtin, Gisela Eisner, Douglas Hall, and Bernard Semmel. It is not a book every scholar will like, but it is a book which every specialist in this period should read.

FRANKLIN W. KNIGHT  
Johns Hopkins University



WILLIAM A. GREEN. *British Slave Emancipation: The Sugar Colonies and the Great Experiment, 1830-1865*. Oxford: Clarendon Press. 1976. Pp. x, 449, \$31.25.

The nineteenth century was a critical period in Caribbean history. Hundreds of thousands of recently-freed Africans and their descendants had to be reconciled to their host society on the basis of legal equality, if not social acceptability. Tens of thousands of indentured aliens joined the debased, denigrated, and exploited laboring populations in a desperate attempt to salvage the failing sugar industry. The fortunes of the plantations produced the most profound effects on the local societies and their relations within their empires and with the wider world. Public policy reflected the paradoxical manifestations of exaggerated nationalism, expansive internationalism, subdued independence, cultural chauvinism, and the aggressive paternalism reflected in the so-called "white man's burden."

These considerations are skillfully reflected in this narration of the political history of the major British Caribbean colonies between two major crises of the age—the abolition of slavery in the 1830s and the reluctant surrender of representative assembly government precipitated by the Morant Bay riot of 1865 in Jamaica. Within its genre—exhaustive Public Record Office research with a metropolitan perspective—this is an outstanding work. It is strong where industry, intelligence, and dispassion are assets and weak where reliance on public reports is a liability. But in a study primarily concerned with the evolution of public policy, the inherent liabilities do not prove disastrous.

The first three chapters describe the society and plantation organization in the British West Indies around the beginning of the nineteenth century. The other chapters provide details on the process of abolition, the travails of the apprenticeship system, the vicissitudes of free labor, the expense of recruiting Africans, Afro-Americans, East Indians, Chinese, and Portuguese, and the peculiar cultural and social institutions of the new polyglot societies. Specialists familiar with the numerous monographs on Caribbean slave societies and the post-slavery period will find little new information here. Non-specialists will find this a particularly welcome and convenient book. William A. Green makes some extremely valuable points often overlooked in the most general references. He emphasizes the variety among the colonies and the resultant anomalies from policies designed especially with Jamaica in mind. He places the estates' economic woes both in a local and an imperial setting and supports the nineteenth-century planter view that lower prices could be offset by larger production. He also illustrates the pervading influence

that the Haitian revolution had on local and imperial politics and race relations.

One obvious shortcoming of this study results from the inability of public policy and public documents to provide adequate insights into the social relations, apart from the planter-initiated indictments of local circumstances. This leads to a number of assertions that do not find support among scholars who have looked outside the Public Record Office, such as Michael Craton and Barry Higman—assertions on local white society, the West Indian great houses, the caste relations, manumission, and the demographic impact which local planters always felt in economic ways. Editorial restrictions preclude a development of these points here, and they are not sufficiently serious to nullify the usefulness of this book.

FRANKLIN W. KNIGHT  
*Johns Hopkins University*

JOHN P. HOOVER. *Sucre, soldado y revolucionario*. Translated by FRANCISCO RIVERA. Cumana: Editorial de la Universidad de Oriente. 1975. Pp. 372.

John P. Hoover's *Sucre* is not the "definitive" biography of the great Venezuelan strategist who was Bolívar's right hand, but the writer of the "definitive" biography will start with it if he is wise, because there is much good material here. Hoover has begun the process of humanizing this "perfect knight" about whom Kathleen Romoli wrote that "neither political adversaries nor debunking had ever unearthed a mean or unworthy act." With this humanizing and very solidly documented biography of the "unimpeachable" Marshal of Ayacucho, Hoover has capped his long and meritorious career with the State Department in Latin America, much of it in Venezuela.

Antonio José de Sucre, a Venezuelan, is best known for planning and executing three brilliant and decisive battles at Junín, Pichincha, and Ayacucho. Beyond that, modern readers want to know what he was like as a boy and as a man. Hoover states that almost nothing is known about the young Sucre. His life seems to start abruptly when he joined the wars of independence in Venezuela as a fifteen-year-old officer in 1810. For a little while Sucre fought with Miranda, the aging Precursor, before joining Bolívar, to whom he then gave his total support. Sucre's devotion to Bolívar partly accounts for the difficulty biographers encounter in giving him the separate analysis he deserves.

Hoover's Sucre emerges as a good diplomat and administrator who was also quite human in confiding some of his personal problems to Bolívar. Sucre apparently played a key role in negotiating



the 1820 truce with the Spanish general Pablo Morillo; he also apparently helped Bolívar to convince San Martín, at Guayaquil, to leave to Bolívar the completion of winning independence for Peru and Bolivia.

Writing from his post as Bolívar's administrator of Bolivia, Sucre sounded much like a son writing to a father. Specifically, Sucre wanted Bolívar's permission to go to Quito to marry his fiancé, Mariana Solando. We learn from Hoover that Bolívar, fatherlike, kept refusing (p. 289), so Sucre worked hard in the presidential palace at his reforms, attended official functions and fiestas, but refused invitations to bullfights, cockfights, and most parties. Occasionally, however, he was "human" when he slipped out of the city to the country homes of friends and managed to carry on mild flirtations with persons he described merely as "the countess" or "the girl from the coast." One La Paz lady insisted that Sucre was the father of a son she bore in 1826. The lady's former fiancé and friends stirred up hostility among the local Bolivians against the Colombian "Liberators," whom they called an army of occupation and told to go home.

Eventually Bolívar permitted Sucre to marry by proxy in January 1828. After being wounded while winning a final battle for Bolívar at Tarqui, Sucre joined his bride for a short honeymoon in Quito. Almost immediately Sucre was immersed in in-law problems. He used up his scant savings to pay their debts, he tried to resign once more, he expressed annoyance that his first and only child was a girl and not "a warrior," and he complained about his hernia and his headaches.

From such very personal tidbits, long buried or omitted by earlier panegyrical biographers, Hoover's Sucre begins to emerge as a flesh and blood human, at last!

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GUILLERMO LOHMANN VILLENA. *Los ministros de la audiencia de Lima en el reinado de los Borbones (1700-1821): Esquema de un estudio sobre un núcleo dirigente*. Seville: Escuela de Estudios Hispano-Americanos. 1974. Pp. cxxiv, 200.

*Los ministros de la audiencia de Lima* is a major contribution to both Peruvian history and the history of Spanish colonial administration. The noted Peruvian historian Guillermo Lohmann Villena uses extensive new evidence drawn from archival research in Peru and Spain to demonstrate convincingly that creoles, often from Lima, belonged to Lima's high court throughout the Bourbon period and were numerically predominant for ex-

tended periods of time. Moreover, an overwhelming majority of the ministers, whether born in America or Spain, married American women, many of whom were *limeñas*. The *audiencia* ministers thus formed part of Peru's nobility, a nobility that identified strongly with royal authority.

Although Lohmann Villena's conclusions about creole and Peruvian presence and ministers' marriages are not new, they deserve the re-emphasis and the systematic treatment he provides. The family and career information he presents in highly useful short biographies of the 158 men is particularly valuable. An appendix provides similar information for fifty-one sixteenth- and seventeenth-century Lima ministers who married American women. These biographies reveal again the richness of Peru's notarial and parish records and one can only marvel at the painstaking research involved.

The author's investigations in Spanish archives were not equally exhaustive. Overreliance upon published works has resulted in some noticeable errors. Two alleged ministers, Andrés José de Iriarte and Antonio Suárez, were, in fact, never appointed to the *audiencia* of Lima. Of greater significance, Lohmann Villena never explains his criteria for considering a person an *audiencia* minister or including individuals in the biographical sketches. One can argue that up to thirty-five of the 158 ministers studied should have been omitted, a change that would alter many figures provided in the text. It is highly questionable, for example, whether men who simply held "honors" of an *audiencia* office or interim appointments should be counted as ministers.

These weaknesses prevent the book from being a definitive study of the *audiencia's* personnel, but do not seriously affect its generalizations. Every student of Bourbon rule should read the lengthy introduction. The biographies forming the core of the book yield much useful and previously unavailable information to specialists in Peruvian history. One hopes that the distinguished author will now turn to a full examination of the impact of the ministers' local ties on government in colonial Peru.

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JAMES PETRAS and MORRIS MORLEY. *The United States and Chile: Imperialism and the Overthrow of the Allende Government*. New York: Monthly Review Press. 1975. Pp. xvii, 217. \$4.50.

The Chilean "peaceful road to socialism" under President Allende (1970-73) has been interpreted

in the most contradictory ways, ranging from extreme-left denunciations of the bourgeois character of the Allende government to extreme-right diatribes against a world-wide Communist conspiracy. The work of James Petras and Morris Morley belongs to the long series of publications that blame the U.S. for the collapse of the Chilean experiment, but it is much better documented than most of the literature in this category. The authors, drawing on predominantly U.S. sources (newspapers, congressional hearings, records of international agencies, personal interviews) set out to prove that the Nixon administration led a many-sided campaign against Chile, and that this campaign, not internal problems, caused the overthrow of the Popular Unity government.

In the first chapter, Petras and Morley discuss the U.S. financial support that helped Frei defeat Allende in the 1964 presidential election. They then detail Kissinger's strategy before and after the September 1970 election. In the next chapter, they place U.S. policy toward Chile in a broader context by surveying U.S. reactions to Brazilian and Peruvian developments in the 1960s and 1970s. In subsequent chapters the authors describe U.S. economic policies vis-à-vis Chile, including credit squeeze, response to the nationalization of the Anaconda and Kennecott copper mines, and renegotiation of the Chilean foreign debt.

To date the evidence documenting the American anti-Allende policies is so overwhelming (there were several new revelations in the U.S. press after this book was published) that no one can seriously doubt the U.S. attempts to prevent Allende's election and later to undermine his government. As a portrait of American interference in Chilean affairs, *The United States and Chile* is a very useful book, both for specialists and general readers, because it assembles a great amount of data and contains an impressive bibliography.

However, the authors' thesis that the U.S. played a paramount role in the overthrow of the Allende regime is clearly an expression of partisan conviction. The acknowledgement of large-scale U.S. involvement in Chile does not justify the conclusion that this was indeed the main cause of the coup in September 1973. The hostile attitudes and actions of American politicians and businessmen do not prove that the Popular Unity leaders could not have pursued wiser, more consistent, and far-sighted policies that possibly might have prevented the tragic outcome.

JIRINA RYBÁČEK-MLÝNKOVÁ  
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STEFAN DE VYLDER. *Allende's Chile: The Political Economy of the Rise and Fall of the Unidad Popular.*

(Cambridge Latin American Studies, number 25.) New York: Cambridge University Press. 1976. Pp. xi, 251. \$13.95.

ROBINSON ROJAS SANDFORD. *The Murder of Allende and the End of the Chilean Way to Socialism.* Translated by ANDRÉE CONRAD. New York: Harper and Row. 1975. Pp. x, 274. \$10.95.

ARMANDO URIBE. *The Black Book of American Intervention in Chile.* Translated by JONATHAN CASART. Boston: Beacon Press. 1975. Pp. x, 163. Cloth \$9.95, paper \$3.95.

Scholarship and passionate commitment to an ideology rarely mix. Treatments by eyewitnesses of the 1970-73 Chilean experiment with Marxism demonstrate this once again. Each of the books reviewed here reflects personal, political, or professional bias. Only Stefan De Vylder's has scholarly merit; the author, a Stockholm-based economist, is well versed in the techniques of his discipline. Armando Uribe and Robinson Rojas Sandford are ideologically committed Chileans. Rojas is a journalist, and Uribe held several diplomatic posts in the administration of Salvador Allende. All three were personally acquainted with Chile during the Allende years.

Future scholars, more objective than many of their predecessors and armed with hindsight, may consider the domestic and international problems of the Allende administration to be as complex as those that plagued the Spanish Republican governments just prior to the Civil War of 1936-39. In so doing they will need to assess the significance of internal disunity and the lack of consensus policy that characterized the leftist coalition—Popular Unity (Unidad Popular, UP)—that supported Allende, in juxtaposition with the domestic and foreign opposition forces. Each of the works reviewed here indicates the necessity for such an approach.

De Vylder makes a convincing case for Allende's inability to carry out the economic transformation of Chile. Making judicious use of statistical evidence and displaying a sophisticated grasp of developmental theory, he carefully assesses the various ways in which lack of consensus on socioeconomic policy hobbled a controversial, extremist regime that was confronted with powerful enemies. He provides his readers with a view of policies within policies, of conflicts within conflicts that eroded the UP coalition and facilitated the blocking of programs by the centrist and rightist parties and by congressional and bureaucratic interest groups. Without going into detail on the opposition, he makes it clear why socialism was not achieved.

Rojas' book is less satisfactory. To Rojas any opposition to Marxism in Chile was virtually treasonous. Although he stood politically to the left of

Allende (precisely where policy conflict was most intense), he regards the "Allendistas" as the only legitimately conceived political group in Chile, as "honorable but very foolish." His curious view of politics from within the UP camp ignores the all too obvious infighting that took place there so that he may concentrate on anti-administration forces.

The Uribe book is unconvincing in dealing with the realities of Chilean socialism and its enemies. Whereas De Vylder recognizes intra-UP dissension as crucial and Rojas ignores it as a matter of convenience, Uribe virtually denies any possibility of its existence. He distorts Chilean history to fit a Marxist mold and blames Allende's tragedy on national and international enemies labeled respectively "traitors" and "imperialists." This is bad history, the author's attachment to Chile's dead president notwithstanding.

In contrast to De Vylder's detachment—bordering at times on the cynical and fatalistic—Rojas and Uribe are ever the partisans. Their purpose is to excoriate the opposition forces. This seriously affects their cause. Rojas writes with the verve of the involved journalist. Wherever he and Uribe may claim the overthrow of Allende was planned, the action nonetheless took place in Santiago, and Rojas has indeed provided a dramatic reconstruction of that action. His version is fascinating reading in some places, but it frequently lacks substance. Having read De Vylder, for example, it is difficult to believe that "honorable but foolish" Allendistas were done in solely by military, bourgeois, imperialist, fascist "murderers and conspirators" controlled by the Pentagon, multi-national corporations, the Department of State, and the Central Intelligence Agency. These undoubtedly formidable foes are monolithic in their commitment and thoroughly coordinated when it suits the author, yet they appear squabbling, divided, and bumbling when that serves his purpose better. No clear relationships between groups in the UP or in the opposition emerge from the book, and it does not occur to Rojas that there might have been either dishonorable Allendistas or opponents with integrity.

Allende neither suffers nor benefits from Rojas' treatment; our knowledge of him does not increase. Moreover, long before it becomes clear that no attempt will be made to fulfill the promise to strip Allende "of the mask of perfection" (p. vii), the reader has grown weary of the author's Manichean approach. Sensationalism simply is no substitute for investigative reporting.

Nor is political experience necessarily a substitute for ability. In a tone at times avuncular, at times shrill, Armando Uribe relates his hostility to the United States and all it stands for. At the age of ten, he tells us, he "began to learn all about the

United States" (p. vii). Uribe's sole purpose is to prove that the United States directly caused the overthrow of Allende. Even had there been no machinations against Allende in this country, Uribe would have conjured them. His *Black Book* purports to be a response to the military government's *White Book of the Change of Government in Chile* (Santiago, 1973), and like the *White Book* it ought to be considered as emotion-charged propaganda. Throughout it offers accusation and innuendo instead of information and thoughtful interpretation. Better efforts have been made to blame Chile's tragedy on "outside influences." Uribe may not be aware of these; his documentation includes nothing that is not available to an undergraduate student. Intelligence gained from the author's apparently extensive diplomatic experiences simply does not figure in his writing, and hyperbole detracts from his argument. His nationalistic antipathy to the United States is even more militant than that of Rojas, but it smacks more of petulance than of indignation.

Each of these books, then, views the 1970-73 episode from related yet unique Marxist-influenced vantage points. De Vylder exposes, Rojas condemns, and Uribe rages. The exposé and condemnation provide interesting reading; the rage does not. Historians will definitely find De Vylder's work worthy of serious consultation. Yet even he yields to ideological temptation when he gratuitously employs a passage from Friedrich Engels' *The Peasant War in Germany* to identify Allende's plight in 1973 with that of Thomas Müntzer in 1525. This alone taxes the imagination, but that Allende's opponents should by extension be made the counterparts of sixteenth-century German princes is preposterous. While all three books merit the attention of those who seek eyewitness accounts of recent Chilean history, only De Vylder's provides an analysis of the complexities of that history.

FREDERICK M. NUNN  
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PEDRO SANTOS MARTÍNEZ C. *Repercusiones de Pavón en Mendoza a través del periodismo (1861-1863)*. Mendoza: Universidad Nacional de Cuyo, Facultad de Filosofía y Letras. 1973. Pp. 508.

This collection of seminar papers, written under the direction of Pedro Santos Martínez C. of the Universidad Nacional de Cuyo, illustrates the kind of training future historians are receiving in the Argentine interior as well as the frustrations experienced by investigators, even native ones, seeking to unravel the history of an up-country province. The aim of the seminar was to assess the effects of two events—the earthquake of March 20, 1861 and

the battle of Pavón (September 17, 1861)—on every aspect of life in the province of Mendoza in the years 1861, 1862, and 1863. Each student closely examined one of the following topics: the earthquake of 1861, schools and the educational system, the press, political parties and their programs, the electoral system, the functioning of the different branches of government, the fiscal system, industry and trade, literature, the Church and its activities, medicine and social welfare, irrigation works and laws, provincial society, and the militia and military campaigns.

The students were to obtain their data primarily from two official provincial newspapers, *El Constitucional* and *El Tupungato*, and the provincial archives. In spite of an extensive search, the newspaper collections proved incomplete, and the available numbers failed to shed much light, for example, on the accomplishments of the governor, the minister, the police force in the departments, and the legislature or on literary activities. The archives likewise were not always helpful, though a few important documents were discovered.

What the students learned from their microscopic examination of Mendoza reinforces the impression that Argentine history, now written from the viewpoint of Buenos Aires, needs considerable revision. The effects of the earthquake were easily identifiable. Schools, churches, hospitals, jails, the official printing press, and some industries were destroyed; the provincial capital had to be moved to a new site; exports to Chile and Bolivia declined; and the geographical distribution of the population was affected.

The same evidence and more suggests that there existed more continuity than discontinuity between the pre- and the immediate post-Pavón years, and that the battle of Caseros (February 3, 1852) was a more important turning point than Pavón. Pavón did not mean the removal of most bureaucrats, the creation of a broader electoral basis, the enforcement of the existing tax laws, a change in relations between the Church and the ultraliberals and the European Masons, the relinquishment of the *real patronato* by the governor, the end of government by an oligarchical minority, freedom of the press, or a politically neutral and more efficient police force. The changes that did occur often seem unrelated to Pavón. The spirit of cooperation that had existed between the Liberals and Federalists since Caseros, for instance, began to disintegrate toward the end of 1863, and the flow of foreigners into the province did not increase until about 1874.

Pavón, the earthquake, the civil disorders of 1861–63, all these did help to deepen an economic crisis which had its origins in the nationalization of the provincial customhouse in 1853, and which

forced the province to become increasingly dependent on national subsidies. Unfortunately, national aid was not always forthcoming or properly applied.

The students evidently wrote the results of their research without comparing notes with their classmates. The consequence is that one paper may contradict another in several respects. This can be disconcerting unless the reader keeps in mind who is examining what. Aside from this defect, this is an excellent volume that should be read, both by Argentine historians and by those interested in the formation of the Latin American states.

JOSEPH T. CRISCENTI  
Boston College

WARREN DEAN. *Rio Claro: A Brazilian Plantation System, 1820–1920*. Stanford: Stanford University Press. 1976. Pp. xv, 234. \$11.50.

The hundred-year cycle of the coffee plantation system in Rio Claro, an export-oriented county in central São Paulo, is laid out skillfully and with analytical verve by Warren Dean. This is the work of a mature scholar whose careful reconstruction of wages and prices and of conditions on the coffee frontier is informed by forthright passion for the underdogs. More than a clear exposition of how the system worked, it is also a spirited indictment of it.

Planters are taken to task for their small vision and their misuse of Brazil's national patrimony: "Income remained low and highly concentrated, in spite of fertile and accessible soil resources" (p. 196). Workers—both slave and free—are seen as the necessary but not inert victims of a rapacious frontier, consolidated early on into a latifundian system in which as late as 1905 fewer than twelve percent of the farm workers labored on their own land. Although some white workers found mobility in the town, the land was a dead end. In Dean's violence-strewn scenario the planters exploit while sometimes adopting a style of self-serving and self-congratulatory paternalism; workers resist as they can, thus showing themselves to be men and women with a usable legacy for contemporaries. To Dean this is what his study of this history means, and even those who distrust or dislike such lesson-drawing have much to ponder.

Dean's *Rio Claro* stands with Stein's *Vassouras* in vigor of argument and range of research. In Dean one finds a more abstract treatment of the *município* in accordance with his emphasis on political economy. If Stein's twenty-year-old classic lacks a chapter on politics and governance, he treats the locality not only as type but also as place, giving more of the pith and qualities of life in this society. In tune with the times, Dean is more forthrightly



speculative. Consider, for example, his statement on page 145 that army officers in the last few months of slavery refused to lead slave-hunting assignments for fear of catching a bullet in the back from their black troops.

In a field as dynamic as the study of plantations and slavery few will claim to have the last word. Dean's book merits high praise, but it is no exception. Recent dissertations by Robert Slenes and Thomas Holloway prove the point. One must now consult Slenes for slave mortality rates, the stability of the slave family, and the uses of paternalism for labor control. The same can be said for Holloway on immigrant mobility. Yet it is to Warren Dean that we turn for the *visita da cúpula*, in short, for the best analysis of the nineteenth-century landed system as a system.

JOHN D. WIRTH  
Stanford University

NANCY STEPAN. *Beginnings of Brazilian Science: Oswaldo Cruz, Medical Research and Policy, 1890-1920*. New York: Science History Publications. 1976. Pp. xi, 225. \$12.95.

This excellent monograph begins by asking "how, when, and why western science began to be established in Brazil." More generally, Nancy Stepan is concerned with the crucial but neglected problem of how science is transferred to those nations which did not participate in the scientific and industrial revolutions. To answer these questions she studied Oswaldo Cruz (1872-1917) and his highly successful Institute.

Stepan outlines the history of Brazilian science, especially medicine, until 1900, when mature, successful science begins. "Successful" science may be defined as an organized institutional effort which helps solve national problems and also produces findings of interest to the worldwide scientific community. The Oswaldo Cruz Institute certainly contributed to such an effort. Beginning modestly in 1900 as the Serum Therapy Institute, charged solely with the production of serums and vaccines, by 1920 the Institute was world-renowned for its investigations of the prophylaxis, etiology, and pathology of tropical diseases.

How did this remarkable transformation occur? A central thesis of the book is that science can be transferred in isolation from the rest of the society if there is "shrewd institution-building" (p. 40) and governmental support. Stepan's account, drawn from the Institute's archives as well as printed sources, shows that Cruz contributed enormously as a well-trained scientist, inspiring teacher, and brilliant administrator. Other factors combined to allow the Institute to flourish: its science—microbiology—was ripe for rapid devel-

opment and met Brazilian needs; it maintained a balance between applied science and pure research, thereby avoiding stagnation; it created clients or consumers for science in government and industry; it employed national, not foreign, scientists; its work was known and appreciated by the elite. None of these factors taken alone explains the success, but taken together they provide a convincing explanation. Stepan confirms these conclusions by comparing the Institute with a similar and coeval institution—the Bacteriological Institute of São Paulo—which atrophied. The final chapter proposes a national science policy for the world's poor nations which will enable them to avoid dependency on imported science and technology.

This book is rich in insights and provokes many questions. For example, even if science can be transferred in isolation, how long can it prosper that way? (The Oswaldo Cruz Institute declined in the 30s.) Are some sciences more transferable than others? What role does a nation's perceived social and economic needs play? Are the biomedical sciences easier to transfer because they threaten no economic arrangements and because their benefits can be easily demonstrated by imposing forced inoculations and sanitation programs on the powerless?

I found Stepan's approach to Brazilian history too narrow and institutional. On balance, however, this is an important book which offers a well-conceived approach to the problem of the transfer of science, and provides much information on a neglected field of Brazilian history and the history of science.

CHARLES O'NEIL  
University of Oklahoma

PETER SEABORN SMITH. *Oil and Politics in Modern Brazil*. Toronto: Macmillan of Canada. 1976. Pp. 289. \$19.95.

Peter Seaborn Smith's subject is the nationalist political struggle that engendered Petrobrás, the state petroleum quasi-monopoly. Smith considers Petrobrás an anomaly, since it was formed before oil was discovered. He seeks to explain that fact as the result of a myth: Brazilians believed their territory was plentifully endowed with deposits which the international oil companies were eager to exploit. In fact, Brazilian geology is not very promising, he says, and the private oil companies were never very interested in prospecting.

The author notices that Petrobrás became a diversified multinational in its own right. It located several small fields, at least, invested in tankers, distribution, and refining, and acquired drilling



rights in other countries. In a postscript he notes that offshore discoveries change considerably the outlook for Brazilian self-sufficiency. He concedes that Petrobrás was the "only viable solution" to the problem of settling the question of Brazil's oil potential but believes the search abroad should have begun earlier. Finally, he is dismayed by the rhetoric of nationalism, apparently not simply because of its irrationalism, but also because of its radical appeal. His preference, apparently, is to leave such decisions to technocrats.

Smith's sources may have determined his focus; they are nearly all newspapers and printed documents. Nevertheless, the concept of public opinion is not developed; a sense of who organized and manipulated and who responded does not come through. Large areas behind the scenes remain unexplored. The Vargas archive, for example, might have revealed more about decision-making. There is little documentation from Petrobrás itself, except for annual reports. The bibliography does not list documents of the private oil companies. Possibly the nationalist position might have appeared more realistic if attention had been given to studies demonstrating illegal activities and interference in Brazilian politics by foreign companies. The author constantly refers to the international oil companies as "the trusts" (in quotation-marks) thereby suggesting that cartelization is also jingoist myth.

It is odd to see a Canadian writing a book like this, but it suggests an interesting paradox: in the capitalist center the myth of abundance proceeds from the right (see Mobil advertising), not the left.

WARREN DEAN  
New York University

GEORGES-ANDRÉ FIECHTER. *Brazil since 1964: Modernisation Under a Military Régime*. Translated by ALAN BRALEY. Foreword by JACQUES FREYMOND. New York: John Wiley and Sons. 1975. Pp. xviii, 310. \$30.00.

This work of contemporary history by a Swiss author familiar with Brazil provides an enlightening overview of the military-dominated governments between the revolution of 1964 and 1972. As a careful and perceptive European, Georges-André Fiechter has succeeded in preserving a high degree

of objectivity. He is obviously sympathetic to the technocrats' attempt to "modernize" Brazil, but he does not hesitate to describe the costs in human terms. Opposition to modernization from the Church and from economic nationalists is clearly presented. The systematic violations of human rights are related, as is the story of the terrorist opposition, which greatly helped legitimize the growing authoritarianism in 1968 and 1969.

Although the approach is primarily chronological, Fiechter also offers brief but well-informed discussions of important new institutions such as the *fundos*, which have become a form of forced saving and a major source of financing for government projects. He also addresses a number of the economic issues that have aroused bitter criticism of the governments of Castelo Branco, Costa e Silva, and Médici. These include wage policy (which resulted in steadily falling real wages until 1968), the proper role for foreign investment (which was given much more favorable treatment than in the earlier 1960s), the use of tax incentives (which were designed to encourage the strengthening of a truly capitalist sector), and the regional impact of growth policies (which appeared to favor the "golden triangle" of Rio de Janeiro-São Paulo-Belo Horizonte).

The author has consulted and cited a large secondary literature as well as contemporary newspapers and periodicals. He emphasizes political struggles and economic policy-making but also discusses the social consequences of Brazil's model of modernization. He clearly spells out the painful dilemma facing the revolutionaries: how to reconcile their commitment to representative government with the need to protect "national security" by authoritarian means.

Fiechter's able synthesis complements Ronald Schneider's *The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964-1970* (1971). Schneider emphasizes political history, especially the involvement of military officers. Like Schneider, Fiechter attempts to apply conceptual models from political scientists such as Rostow and Huntington. Present and future historians will consult with profit Fiechter's balanced assessment of these tumultuous years.

THOMAS E. SKIDMORE  
University of Wisconsin,  
Madison

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

The review of my biography *Stettinius, Sr., Portrait of a Morgan Partner* (not "Business Partner," as the review lists it) by Edwin J. Perkins (June 1976) is puzzling. It contains a number of irritating but not very important misstatements, partial truths, and irrelevant references to supposedly neglected sources. But something further seems to be bothering the reviewer to tie together these apparently random criticisms. I wrote to the reviewer hoping for more light on his views and asked how he proposed to make the following corrections:

1.) The book does not "labor to absolve the Morgan partner" from the Nye Committee (1936) allegation that J. P. Morgan and Co. were "merchants of death." The book only briefly summarizes the events of the abortive Nye hearings, and the review admits that no defense was needed since the "merchants of death" were "an imaginary group of businessmen."

2.) Nowhere do I "candidly admit" that there were misgivings as to whether there was enough material to prepare a genuinely *scholarly* book. More material would have been welcome, but scholarship is not a function of the volume of data.

3.) While certainly a member of the Virginia faculty, the author was not "recruited" to write a volume "honoring" the father of one of the University's most famous alumni.

4.) A biography is not completely uncritical that gives a detailed account of the subject's business failure and use of his clients' funds to speculate disastrously in the stock market.

The reviewer's reply partially explains the review. He apparently believes that a fundamental shortcoming is the book's failure to consider two "crucial issues that twentieth-century American historians want and expect to be addressed and analyzed." The first is Stettinius' involvement with the "merchants of death" theory that J. P. Morgan and Co. helped push this country into World War I to rescue their Allied loans from default. Perkins says: "Indeed by largely ignoring the issue in the body of the text you seem to be indicating that the whole issue should be swept under the rug and forgotten about." He adds, "at times the term 'whitewash' went through my mind."

My response to this is that he can't have it both ways: in the review he has already dismissed the "merchants of death" issue as groundless. As for the wrongdoing that I am supposed to be concealing (in cahoots, he thinks, with the Stettinius family, the University of Virginia, and a kept press), surely he can't be suggesting that Stettinius found time in his eighteen-hour working day as head of the Morgan Allied munitions-purchasing department to foment war between the United States and Germany?

The second issue underlies the final paragraph of the review and concerns how "the role(s) of Stettinius and Morgan relate to the question of the origins of the so-called military-industrial complex." But was there a "military-industrial complex" in the United States in 1915-18? The book shows (from its "biased sources") how Stettinius almost single-handedly created a munitions industry in this country by getting unwilling manufacturers to convert from peacetime production to

munitions-making to fill the orders from the Allies. When the U. S. entered the war, this industry was soon put under government supervision with civilian control. Stettinius, to his chagrin, was prevented by the anti-Morgan Wilson administration from taking more than a very minor part in the U.S. war effort. After the war, the overwhelming number of American munitions manufacturers immediately reconverted to peacetime production, and war industry virtually disappeared. Both before and after this country entered the war, there was liaison between the manufacturers and their ultimate customers, the military, in the technical area. Did this constitute a "complex"?

Of course, my own criticism of the book is that I couldn't find enough of Perkins' "trivia" and "memorabilia"—the human bits—about Stettinius to bring him as fully to life on the pages as I should have liked.

Perkins made the excellent suggestion that I refer readers to reviews of the book expressing other opinions. Accordingly, I call attention to the review in the *Business History Review* (Spring 1975) by Vincent P. Carosso of New York University.

J. D. FORBES  
*University of Virginia*

TO THE EDITOR:

J. D. Forbes wrote without thoroughly consulting

all the pertinent secondary sources, and with predictable results. He recounts the events in Stettinius' life, but makes little effort to place his subject in historical perspective. Moreover, contrary to the author's assertion, the inadequacy of primary data does bear heavily on the scholarly potential of any biography. A senior member of the graduate business school faculty, Forbes does not seem to appreciate the extent to which business history has joined the mainstream of historical scholarship on twentieth-century America over the last two decades. I found it inexcusable that the biography of an important member of the Morgan banking firm, published by a major university press, dealt so slightly and ineffectively with the relationship between government and business during the World War I era, since I felt that the opportunity to gain greater understanding of this organizational interaction (whatever its nature) would be the primary reason why most professional historians might be interested in reading at length about Stettinius.

Of course readers uncertain about the usefulness of this biography for their own teaching and research activities should sample other opinions, but I do not feel that I have misled colleagues about the scholarly value of this book.

EDWIN J. PERKINS  
*University of Southern California*

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## Recent Deaths

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C. GROVE HAINES, a modern European diplomatic historian and an innovative educator, died on May 25, 1976 at the age of 69 in his home at Colle, Val D'Elsa, Italy. In 1972 Haines retired as director of the Bologna Center of the Johns Hopkins School of Advanced International Studies and became professor emeritus. Survivors include his widow, the former Dorothy Sandler, and a daughter by a previous marriage.

A native of Abbottstown, Pennsylvania, Haines received his B.A. from nearby Ursinus College in 1927 and his M.A. (1928) and Ph.D. (1933) degrees from Clark University. He specialized in Italian diplomatic history. In keeping with much of the scholarship of the time, when historians concerned themselves with the background and causes of World War I, he wrote his doctoral dissertation on "Irredentism and Italian Foreign Policy, 1866-1882."

Grove Haines' academic career was divided between the Maxwell School of Citizenship at Syracuse University (1931-45) and SAIS, the Johns Hopkins School of Advanced International Studies (1945-72). At both institutions he enjoyed the reputation of being an imaginative and provocative teacher and an industrious scholar. During World War II, Haines took charge of the Area and Language Training Program at Syracuse, served as assistant to the head of the Foreign Policy Association, and was a member of the United States Foreign Service.

But the most important part of Haines' career came in the postwar era. In 1955 with the cooperation of Rector Felice Battaglia and other officials of the University of Bologna, Haines founded the Bologna Center of the Johns Hopkins SAIS, the first graduate school of an American university ever to be established in Europe. Housed in a building owned by the University of Bologna and located in the heart of the university community, the center was opened with medieval pomp and ceremony. Designed to serve both European and American students, the school's program was geared to contemporary political, economic, and

diplomatic problems with special emphasis on European integration. It did not award advanced graduate degrees. Students were expected to leave the center after a year's study, prepared to enter careers in government, industry, and international organizations.

The Bologna Center's beginnings, as well as its budget, were modest—eleven students and three faculty members. While some growth was contemplated, SAIS officials in Washington were hardly prepared for what followed. Not only did Haines personally recruit able postbaccalaureate young men and women from all over Europe, but he also raised a sizable amount of fellowship and grant money from European business and governmental sources. By 1961 the Bologna Center had outgrown its original quarters and moved up the street to a new modern building which quickly became a Bologna showpiece. Its construction was financed by the United States government under Public Law 480. Fulbright fellowships supported both a number of students and visiting faculty from the United States. Teaching at the Center, as I and others can attest, was a highly challenging and often exciting experience.

Curiously enough, the Communist government of Bologna took pride in having the Center located in the city and never failed to provide it with police protection during periods of student unrest. Such occasions were naturally a cause of concern to Haines, but fortunately they occurred relatively infrequently. A chronic worry was the Center's precarious financial condition. With very modest help from the parent SAIS, Haines succeeded in one way or another in raising enough money each year to keep the Center functioning. His efforts in this regard bordered on the heroic, since financial contributions from European sources proved increasingly difficult to come by.

Enjoying an enviable reputation in Western Europe, Grove Haines was acquainted with many of the leaders of government, industry, finance, and education in virtually every one of the Common Market countries. He was an honorary professor of

the University of Bologna and received awards in recognition for his work from the governments of Italy, Austria, and West Germany, as well as a Tribute of Appreciation from the United States Department of State. Largely through his wide personal contacts, the Center attracted guest lecturers from all over the continent.

Directing an institution such as the Bologna Center demanded infinite tact and patience, and Grove Haines had more than his share of both. Confronted frequently by students making impossible demands and by prima-donna professors who constantly complained of one alleged deficiency or the other in the administration of the Center or the performance of the students, Haines always displayed a remarkable ability to preserve the social and intellectual peace. Those who were associated with the Bologna Center—from library stack attendants to members of the permanent and visiting faculty—were all part of an intimate academic community such as one rarely finds today.

But the reputation of Grove Haines does not rest merely on his ability as an academic administrator. He was also a fine scholar and an able editor as the following partial bibliography of his works attests: *The Development of Western Civilization*, coauthor with Warren Walsh (1941); *The Origins and Background of the Second World War*, coauthor with Ross J. S. Hoffman (1943); *The Threat of Soviet Imperialism* (1954); *What Future Europe?* (1957); ed. *European Integration* (1957); and *Africa Today* (1959).

HOWARD H. QUINT

University of Massachusetts,  
Amherst

JOHN TATE LANNING, James B. Duke professor of history emeritus at Duke University, died on August 15, 1976 at the age of 74. Born and reared in North Carolina, he was graduated from Trinity College in 1924. With advanced degrees from the University of California, Berkeley, Lanning returned to his alma mater (now Duke University) as an instructor in 1927 and taught there until his retirement in 1973. A prodigious scholar, Lanning early reflected the strong influence of his mentor, Herbert E. Bolton, and his own deep attachment to his native Southeast by publishing two books on borderland history—*The Spanish Missions of Georgia* (1935) and *The Diplomatic History of Georgia* (1936). At this juncture, however, Lanning shifted his research interest to concentrate on the intellectual and cultural life of Spanish America. In 1940 his publication of *Academic Culture in the Spanish Colonies* established him as an innovative researcher and able stylist. Fifteen years later he culminated his efforts in intellectual history with two comprehensive works—*The University in the Kingdom of Guate-*

*mala* (1955) and *The Eighteenth-Century Enlightenment in the University of San Carlos de Guatemala* (1956). The latter book won the prestigious Carnegie and Beveridge Prizes of the American Historical Association and the Herbert E. Bolton Prize of the Conference on Latin American History. As complements to these two works, Lanning also published two volumes of documents, *Reales cédulas de la Real y Pontificia Universidad de México* (1946) and *Reales cédulas de la Real y Pontificia Universidad de San Carlos de Guatemala* (1954), which fulfilled his deeply felt obligation to share the results of his work with his Latin American colleagues. The study of medical practice in the Spanish Indies consumed Lanning's last twenty years. In 1974, in the midst of his first bout with bone cancer, he published *Pedro de la Torre, Doctor to Conquerors*. Calling it a mere footnote to his major work, Lanning nevertheless provided a brilliant foretaste of the massive manuscript on the royal *protomedicato* he was preparing at the time of his death.

In his long career John Tate Lanning garnered most of the important awards and recognition that come to distinguished historians. He received fellowships from the Guggenheim Foundation, American Council of Learned Societies, Social Science Research Council, Rosenwald Foundation, and National Library of Medicine; was an elected member of both the *Sociedad Cubana de Estudios Históricos e Internacionales* and the *Sociedad de Geografía e Historia de Guatemala*; was managing editor of the *Hispanic American Historical Review*, 1939–45; and was twice chairman of the Conference on Latin American History. In 1958 the Academy of American Franciscan History gave him the Junípero Serra Award; in 1960 the University of Arizona gave him the Medal of Merit; and in 1961 Duke University honored him with a James B. Duke Professorship. An active member of the American Historical Association, Lanning served as Chairman of both the Albert J. Beveridge Prize Committee and the Nominating Committee.

Although a scholar of international repute, he was also an excellent teacher. During his career he taught over 5000 undergraduates at Duke and trained almost two dozen Ph.D. students. Formidable, stern, and demanding, he set high standards for his students; yet he had a warmth and understanding that won him the abiding affection of those he taught. Lanning also played an important role in shaping the character of Duke University during his early years with his emphasis on high academic standards and scholarly excellence. For many years he was chairman of both the Duke University Research Council and the Council on Hispanic Research. Largely through his efforts the Latin American collection of the Perkins Library



became one of the best in the United States, and his students have honored him by establishing the John Tate Lanning Book Fund.

JOHN J. TEPASKE  
Duke University

JOHN LEDDY PHELAN, professor of history at the University of Wisconsin-Madison, died suddenly on July 24, 1976. Just turned 52, he was at the height of his powers and had already proved himself a distinguished historian of colonial Latin America. Born July 19, 1924 at Fall River, Massachusetts, he earned an A.B. cum laude at Harvard in 1947, then moved to the University of California, Berkeley, for an M.A. in 1948 and Ph.D. in 1951. His doctoral dissertation began a life-long interest in the conceptual matrices that determine men's reactions. The specific topic, the millenarian dreams of sixteenth-century missionaries for a kingdom of God among the Indians, untainted by Spanish civilians or the secular clergy, came from Ramón Iglesia through two close friends of the latter, Edmundo O'Gorman and Lesley Byrd Simpson, who knew of Iglesia's last and lost work on the ideas of Gerónimo de Mendieta. *The Millennial Kingdom of the Franciscans in the New World: A Study of the Writings of Gerónimo de Mendieta (1525-1604)* (Berkeley and Los Angeles, 1956) explores these millenarian dreams, already unreal in Mendieta's last years. A much rewritten version was published as a second edition in 1970, and a Spanish version in Mexico City in 1972.

Because the early 1950s were a period of austerity in Latin American studies, Phelan spent a year on fellowship in France and two and a half at the Newberry Library. At the Newberry he participated in the Philippine program and chose for his topic the imposition of European domination and culture upon a non-European society. *The Hispanization of the Philippines: Spanish Aims and Philippine*

*Responses, 1565-1700* (Madison, 1959), is a subtle, perceptive study of differing conceptions, socioeconomic pressures, and eventual results in the Philippines, but it also steadily compares the Philippines with Mexico, another great laboratory of Europeanization.

In 1956 Phelan entered upon a regular teaching post at the University of Wisconsin-Milwaukee; four years later he moved to Madison. It was his home and spiritual haven for the remaining years of his life. He found Madison in intense intellectual ferment, with special stress upon interdisciplinary cooperation and tropical studies, matters that accorded well with his interests. His future writing was foreshadowed in "Authority and Flexibility in the Spanish Imperial Bureaucracy," (*Administrative Science Quarterly*, 5 [1960]: 47-65), which held that the practices of the Spanish imperial administration represented an effective and sophisticated solution to the problems of governing, with slender resources, widely disparate dominions at far remove from Spain. A major embodiment of this theory is *The Kingdom of Quito in the Seventeenth Century: Bureaucratic Politics in the Spanish Empire* (Madison, 1967). A further inquiry into the other side of the coin, namely, the conditions in which the Spanish administration came to crisis, is *The People and the King: The Comuneros of New Granada (1781)* (Madison, 1977). This, his last book, holds that the Comuneros, far from being revolutionaries, followed traditional Spanish ideas of polity as propounded by such theorists as Francisco Suárez. If hero there was, it was Archbishop Caballero y Góngora, who skillfully reconciled popular demands with the fiscal needs of the crown. There was still much unstruck metal in that mint when it ceased to function.

WOODROW BORAH  
University of California,  
Berkeley

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## Festschriften and Miscellanies

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

LOUIS CAROLUS-BARRÉ, editor. *Septième Centenaire de la Mort de Saint Louis: Actes des Colloques de Royaumont et de Paris, 21-27 mai 1970*. Paris: Société d'édition "Les Belles Lettres." 1976. Pp. ix, 388. 107 fr.

PIERRE-MARIE AUZAS, Essai d'un répertoire iconographique de saint Louis. MARGUERITE BOULET-SAUTEL, Jean de Blanut et la conception du pouvoir royal au temps de Louis IX. ROBERT BRÄNNER, Saint Louis et l'enluminure parisienne au XIII<sup>e</sup> siècle. LOUIS CAROLUS-BARRÉ, La grande ordonnance de 1254 sur la réforme de l'administration et la police du royaume. RAYMOND CAZELLES, Le Parisien au temps de saint Louis. YVONNE DESLANDRES, Le costume du roi saint Louis, étude iconographique et technique. LE R.P. MARIE-ANSELME DIMIER, La place de Royaumont dans l'architecture du XIII<sup>e</sup> siècle. YVES DOSSAT, Alfonso de Poitiers et la préparation financière de la croisade de Tunis: les ventes de forêts (1268-1270). JEAN FAVIER, Les finances de saint Louis. MARIE-MADELEINE GAUTHIER, Les couvertures précieuses des manuscrits à l'usage de la Sainte-Chapelle. DIETRICH LOHRMANN, Pierre Lombard, médecin de saint Louis. Un Italien à Paris et ses maisons au quartier latin. JEAN LONGNON, Les vues de Charles d'Anjou pour la deuxième croisade de saint Louis: Tunis ou Constantinople? JEAN RICHARD, La politique orientale de saint Louis. La croisade de 1248. MARCEL THOMAS, L'iconographie de saint Louis, dans les *Heures de Jeanne de Navarre*. MARIE-THÉRÈSE D'ALVERNY, La connaissance de l'Islam au temps de saint Louis. LE R.P. JACQUES, GUY BOUGEROL, Théologie et spiritualité franciscaine au temps de saint Louis. LE R.P. YVES CONGAR, L'Église et l'État sous le règne de saint Louis. ÉTIENNE DELARUELLE, Saint Louis devant les Cathares. MICHEL-MARIE DUFREIL, Le roi Louis dans la querelle des mendiants et des séculiers (Université de Paris, 1254-1270). RAOUL MANSELLI, Joachim de Flore dans la théologie du XIII<sup>e</sup> siècle. PIERRE MICHAUD-QUANTIN, Le droit universitaire au XIII<sup>e</sup> siècle. BRUNO NEVEU, Le Nain de Tillemont et la *Vie de saint Louis*. JEAN-CHARLES PAYEN, Littérature et chrétienté sous le règne de saint Louis:

équilibres et malaises. LE R.P. MICHEL RIQUET, Saint Louis roi de France et les juifs. LE CHANOINE VAN STEENBERGHEN, "Averroïsme" et "Double vérité" au siècle de saint Louis. MAURICE DE GANDILLAC, Politique et sainteté. JEAN GUITTON, Sur saint Louis. LOUIS CAROLUS-BARRÉ, L'apport historique de l'année saint-Louis, "Le meilleur Roi qui fût au monde."

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## Other Books Received

BOOKS LISTED WERE RECEIVED BY THE *AHR* between December 1, 1976 and February 1, 1977. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

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- RANDALL, JOHN HERMAN, JR. *The Making of the Modern Mind: A Survey of the Intellectual Background of the Present Age*. Foreword by JACQUES BARZUN. Fiftieth Anniver-

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- WELCH, CLAUDE E., JR., editor. *Civilian Control of the Military: Theory and Cases from Developing Countries*. Albany: State University of New York Press. 1976. Pp. xii, 337. \$20.00.
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- MANNING, A. F., and KERSTEN, A. E., editors. *Documenten betreffende de buitenlandse politiek van Nederland, 1919-1945: Periode C, 1940-1945*. Volume 1, *10 mei-31 oktober 1940* [Documents Concerning the Foreign Policy of the Netherlands, 1919-1945: Period C, 1940-1945. Volume 1, May 10-October 31, 1940]. (Rijks Geschiedkundige Publicatiën, Major Series, number 157.) 's-Gravenhage: Martinus Nijhoff. 1976. Pp. xcv, 594.
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- KIEMPERER, KLEMENS VON. *Ignaz Seipel: Staatsmann einer Krisenzeit*. Translated by MARGRETH KEES. Graz: Verlag Styria. 1976. Pp. 381. S 495.
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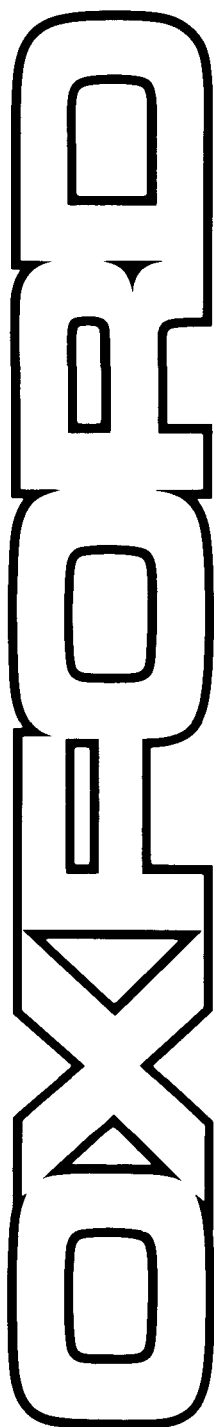
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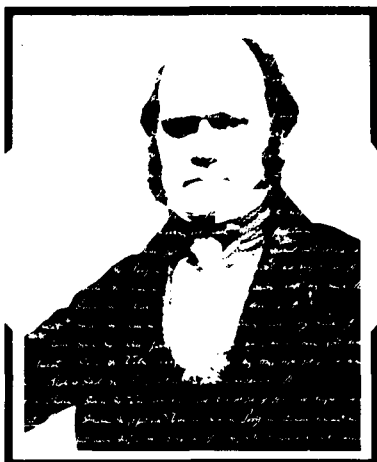


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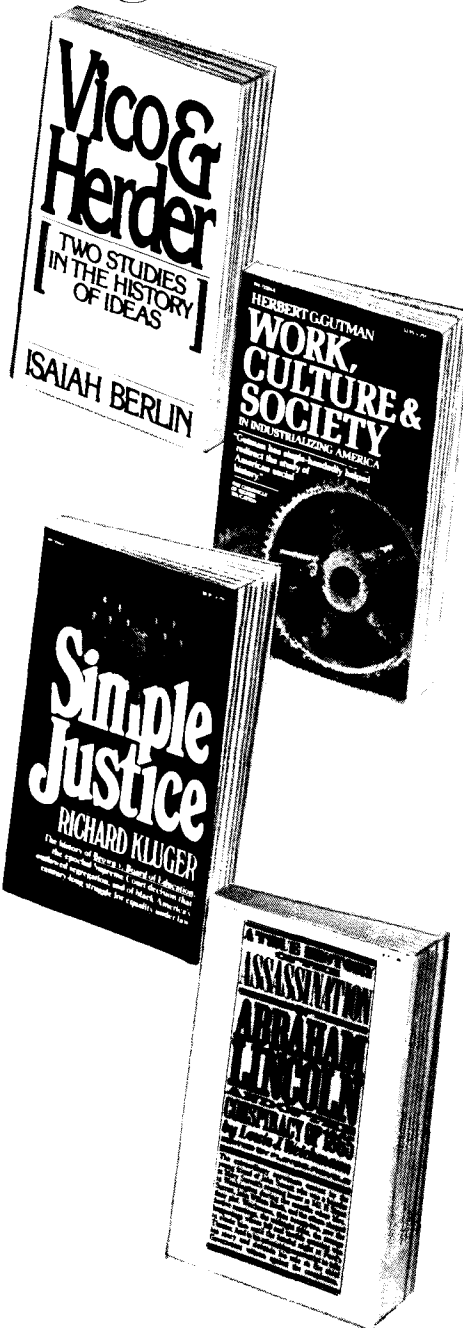
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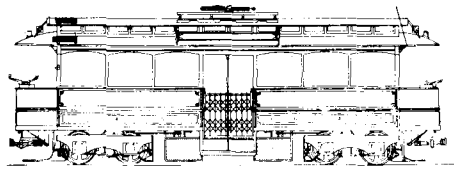
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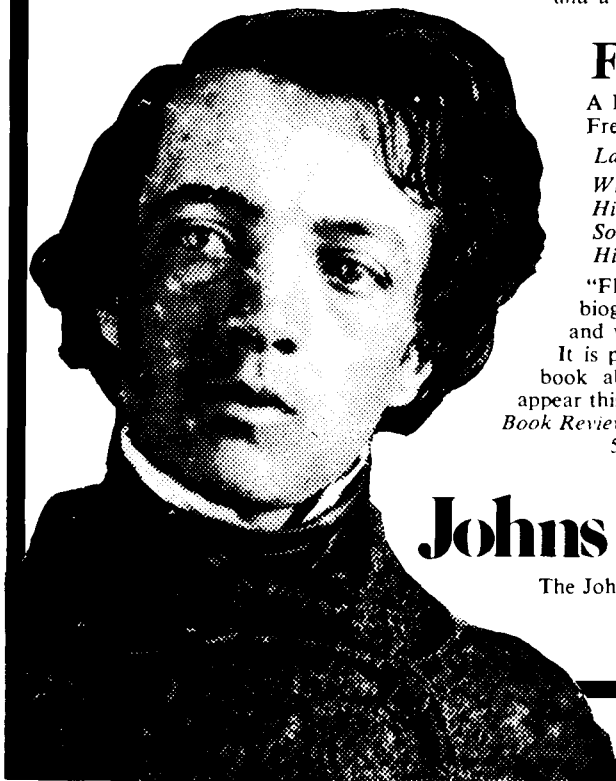
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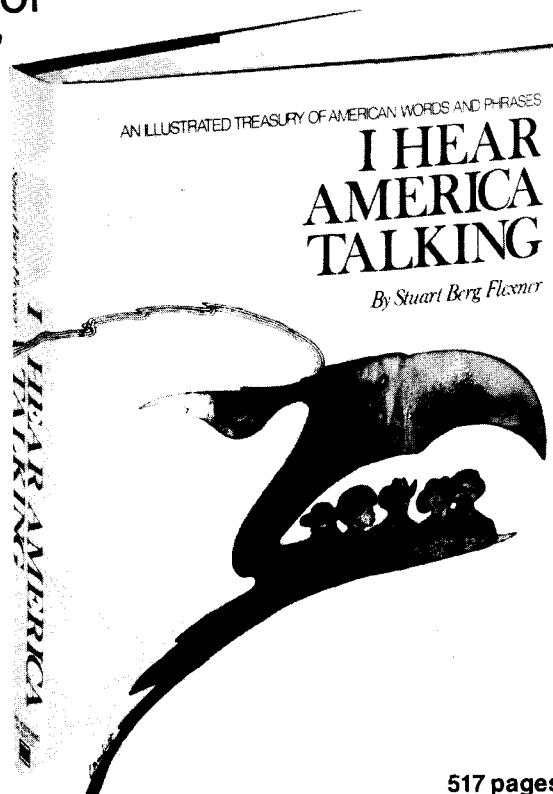
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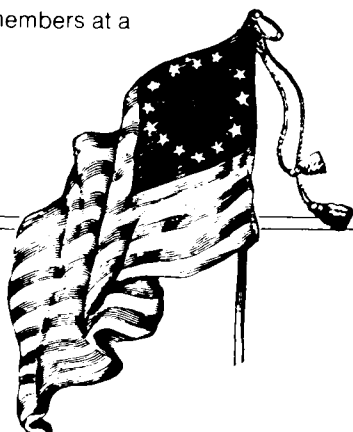
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